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ABSTRACT

Two papers are presented in this newsletter: one on the political activity of scientists following World War II as a result of the Manhattan Project, and one on the political activity of scientists today. These papers are followed by two commentaries which compare the two papers. Also included in this issue are news items, a calendar of events, a report on a conference on ethical guidelines for research and clinical perspectives on human sexuality, an annotated bibliography, and an index to the previous eight issues of this newsletter. (BB)

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[illegible] Kimball Smith

[illegible]ists in an Adversary Culture: the 1970s

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"Focus on the Politics of American Science,"
by Daniel J. Kevles

[illegible] of the Scientific 'Community'"
by Peter Buck

[illegible]itions to the General Bibliography

[illegible]ues 17-24 (October 1976 - June 1978)

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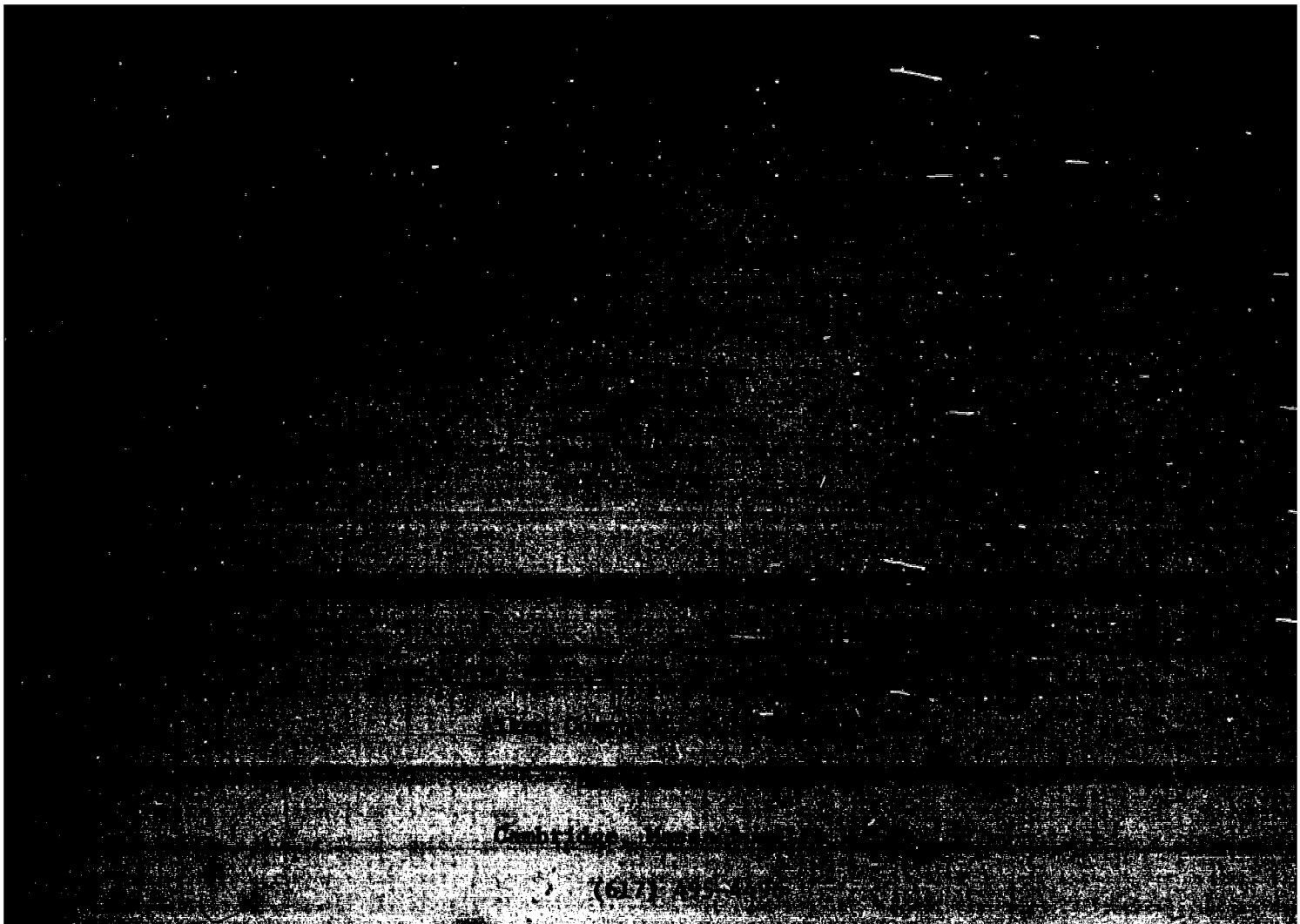

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NEWSLETTER ON SCIENCE, TECHNOLOGY & HUMAN VALUES

June 1978 Number 24

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THE MODEST PRICE OF MATURITY

At its April meeting, the Newsletter Advisory Board recommended the initiation of a subscription fee for the 1978-79 publication year. That recommendation has now been approved by our sponsor, the National Endowment for the Humanities; the rates are listed below. While I adhere to the view that an editor, like a good theatrical director, should remain unseen by the audience served, I believe that our readers are entitled to know the reasons that underlie this decision.

Since its inception in 1972, this publication has enjoyed the financial (and moral) support of the National Science Foundation, The Commonwealth Fund, and the NEH. The magnitude and longevity of this support attest to the importance of our central missions: to serve as a forum for discussion of ethical issues in science and technology; to provide needed scholarly and resource-oriented materials; to act (in the words of the publication's founder) as a "central switchboard and matchmaker for the ... 'invisible college' of producers and users interested in [this] work;" and to reach out to the widest possible audience of interested scientists, humanists, and policy makers.

In the last six years, the field of science-technology-values (STV) has reached a new level of maturity; evidence includes the rapid introduction of STV courses and programs in academic institutions; the proliferation of research conferences, public symposia, legislative hearings, and funding opportunities; and signs of professionalization - e.g., the formation of specialty groups within existing organizations, as well as the birth of new groups with an STV emphasis.

The decision to charge a modest fee for the Newsletter stems from the convictions that the STV field is now sufficiently large and mature to provide at least partial support for the endeavor, and that total dependence on funding agencies is no longer necessary or desirable.

While support from the NEH is assured for another year, the fee is being initiated in 1978-79 in order to increase the likelihood of continuity, to obtain a measure of dependable circulation, and hence to provide an adequate basis for future planning. Reader responses -- in the form of subscriptions -- will serve as the most reliable and meaningful indicator of the demand for continued publication.

Vivien B. Shelanski
Editor

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I. NEWS ITEMS

A. Surveys of Public Attitudes Toward Science/Technology in Europe and the United States

Two surveys of public attitudes toward science and/or technology have been conducted within the past year. Although they yielded results that are in some respects similar, the surveys themselves differed widely in scope, objective, and design.

1) Science and European Public Opinion[1] analyzes nine identical surveys of public opinion in Belgium, Denmark, France, Germany, Ireland, Italy, Luxemburg, the Netherlands, and the United Kingdom--the member nations of the Commission of the European Communities, which sponsored the project. One of the more unusual features of this survey is its sharp distinction between science and technology and its effort to focus solely on attitudes about science: the report's introduction states explicitly that the subject of the study is "research, i.e., scientific activity not directly linked to a result whose effect can be measured..." (p. 2). This focus is reflected in the 19 questionnaire items, which requested opinions about "scientists," "scientific research," "scientific discoveries," and "applications of science," but not about technology or technological developments. [In contrast, other surveys, such as the Harris survey discussed below and the one reported in Science Indicators 1976,[2] do not maintain that distinction. The poll commissioned for the Science Indicators volume, for example, contains such questions as "Have science and technology changed life for the better or for the worse?" "Do science and technology change things too fast, too slowly, or just about right?" and "When science and technology cause problems, who is most at fault?"]

The European study was restricted to problems of particular interest to its sponsors:

- the future of research and the advantages of "European" research;
- scientific activities regarded as having priority;
- the impact of scientific activity and its image in European societies; and
- the interest in science and scientists.

While the attitudes toward science expressed by the 9000 persons interviewed for the survey are by and large quite favorable, a more striking result is the continuity of opinion across socioeconomic class and country; the consensus appears to be independent of the variables that normally influence opinion--e.g., age, level of education, political view.

2) In the United States, approximately 1500 adults were interviewed in November 1977 by The Harris Survey for a study on "Scientific Research & Technology."[3] Respondents were asked whether they agreed or disagreed with a number of characterizations of "scientific research and technological development," e.g., whether they "are necessary to keep the country prosperous," "are the main factors in increasing productivity," "are the real basis of our military strength," "are the only way we can create enough jobs for people who need them," or "tend to overproduce products, and this is wasteful."

Among the results:

- By a majority of 69% to 16%, respondents believe that "scientific research and technological advances" are "the main factors in increasing productivity;" a narrower plurality of 44% to 42% believes that such advances are "the only way we can create enough jobs for people who need them." The Harris organization interprets these results as "evidence that points to a reversal of the traditional connection that long has been made between scientific progress and the growth of job opportunities."

- A majority of 64% to 21% believes that "scientific research and technological development" provide "the real basis of our military strength." According to Harris, "this result reflects the comeback in recent years of public support for defense spending and the need for the United States to have the most advanced technology in the country's military establishment."

- By 52% to 36% percent, a majority of Americans feels that "scientific research and technological development... tend to overproduce products, and this is wasteful." By a narrower 42% to 38%, a plurality believes that this leads to "far too much use of scarce raw materials and natural resources." Harris interprets these responses as a "clear indication... that the applications of future scientific research will have to be far more selective than they were in the past, when raw materials were more abundant and natural resources seemed unlimited."[4]

Questions about the methodology of such studies, as well as the possible uses in policy formation of data obtained through surveys of public attitudes, are being raised with increasing frequency, and will be considered in future issues of the Newsletter. Readers engaged in research on these topics are invited to communicate with the editor.

NOTES

1. Science and European Public Opinion, 97 pages; 1977. Commission of the European Communities, Rue de la Loi, 200, B-1049, Brussels, Belgium.

2. Science Indicators 1976. Report of the National Science Board, 1977. See the chapter, "Public Attitudes Toward Science and Technology," pp. 167-182. (Available from the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402. Stock number 038-000-00341-1; $4.75.

3. Data and quotes are drawn from "Scientific Research & Technology," by Louis Harris, a two-page synopsis of the study released by The Harris Survey on February 27, 1978. Information about this document may be obtained by writing to: Chicago Tribune-N.Y. News Syndicate, Inc., 220 East 42 Street, New York, N.Y. 10017.

4. Methodological questions pertaining to the use and design of surveys to assess public views on issues related to science and/or technology are critically discussed in: T. La Porte and D. Metley, They Watch and Wonder; Public Attitudes Toward Advanced Technologies. Final Report to Ames Research Center, NASA [Grant NGR 05-003-1471] (Berkeley, California: Institute of Government Studies; University of California, December, 1975). T. La Porte and D. Metlay, "Public Attitudes Toward Present and Future Technologies: Satisfactions and Apprehensions," Social Studies of Science 5 (November 1975): 378-384. For discussions of a current controversy about survey methodology and the possible uses and effects of survey data, see: G.B. Kolata, "Polling the Professors: Survey Draws Protest," Science 199 (February 17, 1978): 751-755; and S. Lang, "Strange Survey of US Profs," The New York Review of Books, XXV (May 18, 1978).

B. Carter Administration Proposes Transfer of Some NSF Science Education Programs

Although the fate of the Carter Administration's proposal to establish a new Department of Education remains uncertain, the Administration's views regarding the relation between the NSF Science Education programs and the proposed new Department have recently emerged. Despite the recommendation of the National Science Board and NSF Director Richard Atkinson that all present education programs be retained in the NSF, the Administration has proposed that most of the agency's science education activities be transferred to a Department of Education. Programs that would remain in the NSF according to the Administration's proposal include graduate training and fellowship programs, and the programs of NSF's Office of Science and Society: Ethics and Values in Science and Technology (EVIST), Public Understanding of Science, and Science for Citizens.

The Administration's position was presented to the Senate Governmental Affairs Committee by J. T. McIntyre, Director of the Office of Management and Budget: "We think that a Department of Education responsible for improving educational quality should directly involve science education programs designed to upgrade school and college curricula" (Science Trends, April 24, 1978). Science Trends also reported that, in an April 10 letter to President Carter, NSF Director Atkinson argued that if NSF's science education programs are transferred to the new department, they will represent less than 1/200 of the agency's budget, forming "an anomolous unit that will be too small to be effective."

It appears unlikely that the proposal to establish a Department of Education will receive approval from both the Senate and House of Representatives in this session of Congress. It is possible that it will be reconsidered in 1979, after the November 1978 Congressional elections.

C. New Program to Promote Press Coverage of the Social Sciences

The Center for the Study of Youth Development at Boys Town, Nebraska, recently launched a national effort to promote quality reporting of the social sciences in the mass media. The "Social Science in the Mass Media" project will focus on the development of educational materials and programs for journalists and social scientists through seminars, print materials, and surveys of reporter and scientist attitudes. The Center for the Study of Youth Development is a national research service complex concerned with issues about children, youth and families.

The project coordinator, S. Holly Stocking, has pointed out that journalists and social scientists have long talked of the need for projects that would "upgrade the social science sophistication of reporters and editors and better inform scientists about the mass media dissemination process," yet sustained efforts that focus on the particular problems of the social sciences are rare. In an effort to meet this need, the Boys Town project will develop seminars to help reporters understand basic social science methodology and use information sources in the social sciences, and will prepare glossaries and guidelines for journalists. In addition, project staff will survey attitudes of social scientists toward mass media dissemination of their research, analyze the issues that must be faced by scientists who are disseminating their research results to the public, and develop recommendations for promoting positive scientist-reporter interactions.

As part of the project, Stocking is seeking social scientists' anecdotal accounts of successful and unsuccessful interactions with journalists. Such accounts will be integrated with empirical findings on mass media dissemination of social science research and used in educational materials aimed at the working press and journalism students. Interested readers should contact S. Holly Stocking, Coordinator of Science Writing Projects, Center for the Study of Youth Development, Boys Town, Nebraska 68010, 402-498-1570.

D. EVIST Resource Directory Published By AAAS

A directory of courses and programs related to the field of ethics and values in science and technology has been compiled by the AAAS Office of Science Education under a grant from the National Science Foundation. Listings of more than 900 EVIST-oriented courses are organized under five topical headings: (1) science, technology, and human values; (2) environmental concerns; (3) health care, life science, behavioral sciences; (4) industry, commerce, and society; and (5) public policymaking.

Designed as an update to the 1976 Guide to the STS field prepared at Cornell University [see Issue #21, October 1977, p. 14], the EVIST Resource Directory is based on information provided by respondents to a questionnaire distributed in early 1977. While omissions are inevitable in any undertaking of this kind--there is, for example, no section on EVIST-oriented publications--this is the most comprehensive guide to the field available and should prove valuable particularly to those persons engaged in curriculum development.

The EVIST Resource Directory is available free of charge from the Office of Science Education, American Association for the Advancement of Science, 1776 Massachusetts Avenue, N.W., Washington, D. C. 20036.

E. NSF EVIST Program Announces Awards, New Guidelines

The National Science Foundation's Ethics and Values in Science and Technology (EVIST) Program has announced the following grants (to date) in fiscal year 1978:

Daniel L. Babcock, Department of Engineering, University of Missouri, Rolla, Missouri 65201
"Workship on Values and the Public Works Practitioner"
(Partial support from the NEH)

Nancy N. Dubler, Department of Social Medicine, Montefiore Hospital and Medical Center, Bronx, New York 10467
"Ethical Issues in the Delivery of Health Care Within Detention and Correctional Institutions"

Diana B. Dutton, Department of Family, Community and Preventive Medicine, Stanford University School of Medicine, Stanford, California 94305
"Ethical Issues in Biomedical Decision-Making: Four Case Studies"
(Partial support from the NEH)

William Friedland, Sociology Department, University of California, Santa Cruz, California 95060
"Ethics and Values in Agricultural Research: A Case Study"

Raymond G. Hunt, Institute for the Study of Contemporary Social Problems, Seattle, Washington 98105
"Case Studies of Value Dilemmas in Law Enforcement"

Robert F. Ladenson, Department of Humanities, Illinois Institute of Technology, Chicago, Illinois 60616
"Bibliography of Engineering Ethics"
(Partial support from the NEH)

Everett Mendelsohn, Department of the History of Science, Harvard University, Cambridge, Massachusetts 02138
"International Conference on Ethical and Value Issues in the Social Assessment of Science"

Nicholas H. Steneck, Department of History, University of Michigan, Ann Arbor, Michigan 48109
"Value Issues in Research on the Biological Effects of Microwave Radiation: A Case Study"

Murray L. Wax, Social Science Institute, Washington University, St. Louis, Missouri 63130
"Ethical Problems of Fieldwork"

A revised set of guidelines will be issued by the EVIST Program in mid-June, and may be obtained by writing to: EVIST Program, Office of Science and Society, National Science Foundation, Washington, D.C. 20550.

F. Report on Scholarly Freedom and Human Rights

The Study Group on Scholarly Freedom and Human Rights, a section of the British Council for Science and Society, has published a report focusing on "what can be done now for the protection of scholarly freedom, on the basis of principles, norms and institutions which are already in being" (p. 8), for scientists on an international level. The 63-page report, Scholarly Freedom and Human Rights, acknowledges in its Introduction both the immediacy and the relevance of human rights issues as they affect scientists:

> "Although scientists and scholars are commonly imagined as other-worldly creatures, isolated from the struggles of politics and business, their calling is not without its hazards. The pursuit and proclamation of unpopular truths may provoke a political reaction: Galileo was not the first nor the last celebrated martyr of science." (p. 7)

The report then proceeds through a cogent argument that touches on the conditions necessary to science and on the motives, forms and justifications for oppression of scientists. In a third section, the historical antecedents and legal foundations for current international

human rights law are carefully drawn; the report then takes up the most characteristic issues of scholarly freedom and examines "how they are dealt with in the various documents, noting gaps that are not covered by any existing instruments or declarations." (p. 32). In this manner, the authors address rights peculiarly important to scientists--the right to education and work, protection of scientific "production" processes, freedom of expression/opinion, movement/residence, assembly/association and entry to countries that are the sites of conferences. Then, resting on these discussions of the existing situation and the intrinsic scientific mandates, the report analyzes the "specific" and the "practical," that is, procedures and actions by which to enforce the protection of scientists' rights and freedoms.

In furtherance of its goal to offer "something of immediate practical value to the community of scientists," the Study Group makes three specific recommendations:

> "Both as individuals and through their corporate institutions, scientists should never fail to protest, in private and in public, at the oppression of colleagues for whom they have a special concern.
>
> "Again, both individually and corporately, scientists should subscribe publicly to statements that support the principles of scholarly freedom established by the consensus of International Human Rights Law.
>
> "And the scientific community, through its institutions, should sponsor the establishment of an impartial clearing-house for information on denials of scholarly freedom, and apply appropriate sanctions in cases of established and illegitimate oppression." (p. 63)

To order Scholarly Freedom and Human Rights, write directly to the publisher: Barry Rose, Ltd., Little London, Chichester, Sussex PO19 1PG, England. Order Number K K55526; $8.00.

G. Forum for the Advancement of Students in Science and Technology

Since 1970, the Forum for the Advancement of Students in Science and Technology (FASST) has provided a vehicle for the participation of students in the discussion of science and technology issues. FASST was originally founded by engineering students advocating support of the SST; but membership and direction have since broadened and the organization has metamorphosed from one-issue advocacy to the promotion of education and discussion on all contemporary science and technology issues. The non-profit organization now includes among its several thousand members many social science and humanities students who are interested in the interaction of technology and society and are seeking a greater understanding of the technical content of current social issues. FASST programs aim to supplement traditional classroom

learning, and to bring to the attention of student members the most current issues and events in a broad range of fields, from CETI to rDNA; from human rights to energy alternatives.

FASST publishes a quarterly news magazine (FASST News) and a membership newsletter (FASST Tracks). Frequent press releases and a news service also support education efforts by dissemination through campus newspapers and similar publications. The organization has also recently served as the home for media interns interested in science communication; the staff is actively seeking more interns for work on the publications and news service and would like to hear from interested students or faculty sponsors.

FASST is an affiliate of the American Association for the Advancement of Science. Funding comes from membership dues, project grants and corporate contributions. For further information, contact Alan Ladwig, FASST, 2030 M Street, N.W., Suite 402, Washington, D.C. 20036; 202-466-3860.

H. Call for Papers on Technology in American Culture

A 1979 issue of the Journal of American Culture will feature a section of articles that surveys the current state of technology and culture studies in a manner useful to both generalists and specialists in this field. The editors of this special section encourage the submission of essays from a broad range of perspectives and academic disciplines. PART I will focus on overviews on technology's past, present and/or future roles in the American experience, as well as more specific treatments of the relation between technology and values. PART II will focus on "Technology in Context": a) Social History--Essays focusing on technology and social institutions, patterns of living, and cultural styles; b) Aesthetics, Philosophy and Religion--Essays concerning technology and the symbolic dimensions of culture; c) Public Policy--Essays on environmental issues, technology transfer, and technology and political ideology.

Twelve to fifteen essays will be selected for the issue, which will appear early in 1979. Essays should be 10-15 pages, should include endnotes, and should be sent no later than November 15, 1978, to Professors David E. Wright and Robert E. Snow, Lyman Briggs College, Michigan State University, East Lansing, Michigan 48824.

I. Project to Increase Awareness of Ethical Dilemmas in Nursing

A four-state regional humanities project--"Nursing and the Humanities: A Public Dialogue"--seeks to focus attention on ethical issues specific to nursing through a series of public programs and a regional conference. Supported by grants from the state humanities committees of Connecticut, Massachusetts, Rhode Island, and New York, the project

sponsored a regional conference in November 1977 in which nurses, philosophers, and other humanists met together to explore various dimensions of nursing, and in April and May 1978, held a series of public programs based on issues that emerged at that conference. Interested persons may write to: Sheri Smith, Department of Philosophy, Rhode Island College, Providence, Rhode Island 02908. [From "Nursing and the Humanities Project," American Philosophical Association Committee on Philosophy and Medicine Newsletter, No. 8, April 1978].

J. Undergraduate Conference on "Life, Death, and Human Rights": November 1978.

The Brown Bioethics Association, an undergraduate group composed primarily of biomedical ethics concentrators, is sponsoring a conference on "Life, Death, and Human Rights," to be held at Brown University, Providence, Rhode Island, November 18-19, 1978. Interested undergraduates, medical and nursing students, health care professionals, philosophers, and other persons are invited to attend. Most of the conference will consist of short talks given by undergraduates, on topics to include: personhood; defective infants; microallocation and macroallocation of resources; abortion and euthanasia; and genetics, population and birth control. For registration information, write to: Scott Berman and Donya Powers, Conference Co-Chairpersons, Brown Bioethics Association, Box 1930, Brown University, Providence, Rhode Island 02912.

K. Help for Dissertation Research in Scientific Communication

Annual Reviews, Inc., is the California-based non-profit publisher of twenty-two series of specialized scientific reviews. Since the success of these ventures depends on the voluntary cooperation of authors and the support of each scientific field, the Board of Directors of Annual Reviews is sensitive to the need for expanded research on scientific communication.

The Board has announced that it is receptive to proposals for "graduate fellowships that would encourage and assist dissertation research in scientific communication." The resources of the corporation do not permit the establishment of comprehensive research grants, but the Board welcomes suggestions about "modest seed efforts that could reasonably lead to more substantial support." Although no formal procedures for application have, as yet, been developed, informal comments and suggestions may be sent to William Kaufmann, Editor-in-Chief, Annual Reviews, Inc., 4139 El Camino Way, Palo Alto, California 94306.

II. NEWS FROM THE SOCIETIES

A. Philosophy of Science Association

The Philosophy of Science Association is holding its Sixth Biennial Meeting, October 26-29, 1978, at the Jack Tar Hotel in San Francisco. The program will consist of contributed paper sessions, special sessions and invited lectures, and symposia. The final program will be announced later this summer. The following are the current tentative program arrangements:

Contributed Papers

a) History of Philosophy of Science

Nancy Maull, Yale. "Perception and Primary Qualities."
Edward MacKinnon, California State/Hayward. "The Development of Kant's Conception of Scientific Explanation."
John V. Strong, Boston College. "John Stuart Mill, John Herschel, and the 'Probability of Causes'."

b) Philosophy of Physics

Jeffrey Bub and Vandana Shiva, University of Western Ontario, Canada. "Non-local Hidden Variable Theories and Bell's Inequality."
Nancy Cartwright, Stanford. "The Only Real Probabilities in Quantum Mechanics."
Carlo Giannoni, Rice. "A Universal Axiomatization of Kinematic Theories."

c) Observation and Theory

Kathleen Okruhlik, University of Western Ontario, Canada. "The Interplay Between Theory and Observation in the Solar Model of Hipparchus and Ptolemy."
William B. Jones, Old Dominion. "Theory-ladenness and Theory Comparison."
P. William Bechtel, Northern Kentucky, and Eric Stiffler, Western Illinois. "Quine and the Epistemological Nihilists."
Mark G. Tamthai, University of Chulalongkorn, Thailand. "Analyticity Without an Observational Theoretical Distinction."

d) Case Studies with Applications to the Philosophy of Science

Barbara Klein, Yale. "The Role of Psychology in Functional Localization Research."
Thomas Nickles, Nevada-Reno. "Scientific Problems and Constraints."

Lindley Darden, Maryland. "Discoveries and the Emergence of New Fields of Science."

e) Explanation

Robert Cummins, Wisconsin-Milwaukee. "Explanation and Subsumption."
J.K. Derden, Humboldt State, California. "Reasons, Causes and Empathetic Understanding."
T.R. Giriil, Visalia, California. "Approximative Explanation."

f) Aspects of Rationality

Sandra Harding, Delaware. "Four Contributions Values Can Make to the Objectivity of Social Science."
Caroline Whitbeck, SUNY/Albany. "Four Basic Concepts of Medical Science."
Paul Thagard, Michigan/Dearborn. "Why Astrology is a Pseudo-Science."
Maurice Finocchiaro, Nevada-Las Vegas. "Rhetoric and Scientific Rationality."

g) Quantum Logic

Peter Mittelstaedt, University of Köln, Germany. "The Metalogic of Quantum Logic."
David Holdsworth, University of Western Ontario, Canada. "A Role for Categories in the Foundations of Quantum Theory."
James McGrath, Indiana/South Bend. "Only if Quanta Had Logic."

h) Applications of Statistical Ideas

B. Loewer, R. Laddaga and R. Rosencrantz, San Francisco. "The Likelihood Principle and an Alleged Antinomy."
Andre Mirabelli, Boston. "Belief and Incremental Confirmation of One Hypothesis Relative to Another."
David Papineau, MacQuarrie University, Australia. "Salmon, Statistics and Backwards Causation."

Special Sessions and Invited Lectures (Speakers and/or Commentators listed)

a) Game Theoretic Semantics for the Language of Science--E.W. Stachow, University of Köln, Germany; Esa Saarinen, Finnish Academy.

b) Reduction--Paul A. Bogaard, Mount Allison University, Canada; Arthur Caplan, The Hastings Center, New York; Kenneth Schaffner, Pittsburgh.

c) New Foundations for Quantum Mechanics--Simon Kochen, Princeton; Richard Healey, Cambridge, England.

d) The Psychology of Resemblance--Amos Tversky, Stanford; Nelson Goodman, Harvard.

e) Physical Randomness--Paul Benioff, Argonne National Laboratory, Illinois; Geoffrey Hellman, Indiana; Paul Humphries, Arizona.

Symposia (Speakers and/or Commentators listed)

a) Philosophy of Technology--Paul Durbin, Delaware; S.C. Thakur, Surrey, England; Henryk Skolimowski, Michigan.

b) Statistical Mechanics--John Earman, Minnesota; David Melament, Chicago; Lawrence Sklar, University of Michigan.

c) The New Geology--David Kitts, Oklahoma; Rachel Laudan, Pittsburgh; Michael Ruse, Guelph, Canada.

d) Lakatos' Proofs and Refutations-- William Berkson, Washington, D.C.; Solomon Feferman, Stanford; Arthur Fine, Chicago Circle.

e) Habermas--Mary Hesse, Cambridge, England; Thomas McCarthy, Boston; Cary Gutting, Indiana.

f) Statistical Evidence--Glenn Shafer, Kansas; Isaac Levi, Columbia; Teddy Seidenfeld, Pittsburgh.

g) Laudan's Progress and its Problems--Robert Westman, UCLA; Noretta Koertge, Indiana; Hugh Mellor, Cambridge, England.

h) Conditionals--Brian Skyrms, Chicago Circle; Alan Gibbard, Michigan; John M. Vickers, Claremont.

i) Other sessions--The Unity of Science; Taxonomy.

For further information about the meeting write: PSA Business Office, 18 Morrill Hall, Department of Philosophy, Michigan State University, East Lansing, Michigan 48824.

B. History of Science Society

1. June Meeting of the West Coast History of Science Society

On June 14-15, 1978, the West Coast History of Science Society (WCHSS) will be meeting in conjunction with the Pacific Division of the AAAS in Seattle, Washington. Following is a list of pertinent sessions and papers:

a) June 14, 9:00 a.m.--History of the Biomedical Sciences; Chairperson: Charles W. Bodemer (University of Washington).

E.T. Pengelley (University of California-Riverside) "Places of Historical Association with Major Discoveries in Biology and Medicine;"

Keith R. Benson (Whitworth College) "William Keith Brooks (1848-1908) and the Morphology Tradition in American Biology;"

Robert G. Frank (UCLA) "Adrian and Frequency Coding in the Nervous Systems: Conceptual & Technological Roots of Discovery;"

James Whorton (University of Seattle) "The Pathology of the Wheel" [on early debates over bicycling and health].

b) June 14, 1:30 p.m.-- History of Technology and Technology Policy; Chairperson: Bradford Blaine (Scripps College).

David Hounshell (Harvey Mudd College) "The Expert Syndrome in the History of Technology;"

Leslie Blanchard, "Resistance to 'Outside' Innovation in the British Lighthouse Establishment: 1820-1900;"

Wendy Allen (Rand Corporation) "Technological Innovation and Technology Transfer: Implications for Arms Control."

c) June 15, 9:00 a.m.-- Work in Progress in History of Science; Chairperson: Robert Filner (San Diego State University).

Sharon Traweek (University of California-Santa Cruz) "Science and Culture: Experimental Physics in Japan and the United States;"

Susan T. Frey (University of Washington) "Science and Marxism: The Dialectics of Nature;"

Michael Nutkiewitz (UCLA) "Science and Political Theory: The Laws of Physics in Hobbes and Spinoza;"

Judith Goodstein (California Institute of Technology) "Correspondence Between Scientists: A Selection of Letters from the Caltech Archives;"

Margaret J. Osler (University of Calgary) "Descartes, Gassendi, and the Foundations of the Mechanical Philosophy."

For further information, write Richard Olson, Secretary-Treasurer of WCHSS, Harvey Mudd College, Department of Humanities and Social Sciences, Claremont, California 91711.

2. Symposiums on Teaching the History of Science

a) The Committee on Undergraduate Education of the History of Science Society will sponsor a symposium on "History of Science in the Science Curriculum" at the annual meeting of the History of Science Society,

Madison, Wisconsin, October 27-29, 1978. For further information, write Stephen G. Brush, Institute for Physical Science & Technology, University of Maryland, College Park, Maryland 20742.

b) A symposium on teaching the history of science was held at the annual meeting of the History of Science Society in Dallas, Texas, December 29, 1977. Three speakers described how they successfully introduced undergraduate courses in their own institutions, and distributed detailed outlines and bibliographies. A report in the April 1978 issue of Scan, by Arthur Donovan, includes long abstracts of the three talks by Sheldon J. Kopperl, Lois N. Magner, and Stanislaus J. Dundon.

C. Society for Philosophy & Technology

1. The first volume of the official publication of the group--Research in Philosophy & Technology, vol. 1 (1978)--came out in mid-April. It is available from JAI Press, Greenwich, Conn. 06830. The volume includes papers from the 1975 University of Delaware conference and from a symposium on methodology for philosophy of technology at the 1976 (Boston) AAAS meeting, as well as a bibliographical update by Carl Mitcham and Jim Grote covering mostly items for 1973 and 1974.

2. RP&T-II is in the manuscript stage and should be available in Spring 1979. Contents include: Part I. Historico-Philosophical Studies: Carl Mitcham (St. Catharine College, Ky.), "Philosophy of Technology: Origins and Issues;" Wolfgang Schadewaldt (Late Professor of Classics, Tübingen), "The Concepts of 'Nature' and 'Technique' According to the Greeks;" Jacob Klein (St. John's, Annapolis), "On the Nature of Nature;" Documentation: Jacques Lafitte, Réflexions sur la science des machines (1932), and Simone Weil, Two Uncollected Letters on Machines; Michael Zimmerman (Newcomb College, Tulane), "Heidegger and Marcuse: Technology as Ideology." Part II. Conference and Non-Conference Papers: Albert Borgmann (Montana), "Freedom and Determinism in the Technological Society;" Edmund Byrne (Indiana-Purdue, Indianapolis), "The Normative Side of Technology;" Phillip Fandozzi (Montana), "Art in a Technological Society;" Bernard Gendron and Nancy Holmstrom (Wisconsin, Milwaukee), "Marx, Machinery, and Alienation." Part III. European Contributions: Jozef Banka (Katowice, Poland), "Euthyphronics and the Problem of Adapting Technical Progress to Man;" Hans Lenk and Günter Ropohl (Karlsruhe), "Toward an Interdisciplinary Pragmatic Philosophy of Technology;" Friedrich Rapp (Technical University, Berlin), "The Physical and Social Foundations of Technology." Part IV. Review and Bibliography: Werner Koenne (Vienna), "On the Antagonism Between Philosophy and Technology in Germany and Austria;" Alois Huning (Wülfrath, Germany), "Philosophy of Technology and the Verein Deutscher Ingenieure;" Jean-Claude Beaune (Clermont), "Philosophy of Technology in France, I: Overview and Bibliography 1972-1977;" Daniel Cérézuelle (Grenoble), "Philosophy of Technology in France, II; Ellul, Simondon, Brun;" Lucia Palmer (Delaware), "Philosophy of Technology in Italy;" Frederick A. Rossini (Georgia Tech), "Technology Assessment: A New Type of Science?" K.S. Shrader-Frechette (Louisville), "Kenneth Sayre on Information-Theoretic Models of Mind."

3. RP&T-III, in addition to further bibliographical updates under the direction of Mitcham (whose grant from NSF for that purpose was announced in the last issue of Science, Technology, and Human Values), will include mostly papers presented at the 1977 University of Delaware conference, as well as those read at various SP&T symposia in conjunction with meetings of the American Philosophical Association, 1976-1978. Some additional materials are expected to include a symposium on Jacques Ellul to which he has agreed to contribute.

4. The March SP&T symposium, in conjunction with the APA/Pacific meeting in San Francisco, featured papers by Michael Scriven, UC/Berkeley ("Philosophy of Science vs. Philosophy of Technology") and Robert McGinn, Stanford ("Technology and Intagibles: A Characteristic Dilemma of Modern Technological Innovation"). About 80 people attended, making it one of the society's most successful symposia up to that time.

An April symposium (APA/Western, Cincinnati) was even more successful. Built around a multi-media presentation by architect/philosopher Paolo Soleri, "Soleri's Cities: Energy Conservation through Value Transformation?," the panel included comments by Phillip Fandozzi (Montana), Joseph Margolis (Temple), Henryk Skolimowski (Michigan), and Willis Truitt (South Florida). Approximately 800 people attended, perhaps 100 philosophers and the rest general public.

There is to be a philosophy of technology symposium at the Philosophy of Science Association meeting in San Francisco in October. The topic is science, technology, and contemporary culture. Currently scheduled panelists are Paul Durbin, Henryk Skolimowski, and Shivesh Thakur (University of Surrey, England). Alex Michalos (Guelph, Canada) arranged this symposium. In addition, there will be a symposium at the APA/Eastern meeting in December, arranged by Stanley Carpenter (Georgia Tech), who is still accepting papers.

III. MEETINGS CALENDAR

12 August 1978	Symposium on Teaching Science and Environmental Writing: "The Journalism of Uncertainty," Association for Education in Journalism, Annual Meeting; Seattle, Washington.
14-18 August 1978	Conference on Critical Issues in the History of Technology; Hotel Roanoke, Roanoke, Virginia.
20-25 August 1978	AIBS, 29th Annual Meeting; the University of Georgia, Athens, Georgia.
26 Aug.-2 Sept. 1978	World Congress of Philosophy, Düsseldorf, GERMANY.
28 Aug.-1 Sept. 1978	American Psychological Association, Annual Convention; Toronto, Ontario, CANADA.
4-8 September 1978	Second International Conference on the History and Philosophy of Science; Domus Galileana, Pisa, ITALY.
12-14 October 1978	Mountain-Plains Philosophy Conference, Theme: Contemporary Ethical Problems, University of Colorado, Boulder, Colorado.
19-21 October 1978	SHOT, 21st Annual Meeting; Pittsburgh, Pennsylvania.
26-29 October 1978	Philosophy of Science Society, Sixth Biennial Meeting; Jack Tar Hotel, San Francisco, California.
27-29 October 1978	History of Science Society, Annual Meeting; University of Wisconsin at Madison.
29-31 October 1978	Guy L. Leonard Memorial Conference in Philosophy (following the PSA Biennial Meeting), Topic: Scientific Discovery. University of Nevada, Reno, Nevada.
3-5 November 1978	Society for Social Studies of Science, Third Annual Meeting, Indiana University, Bloomington, Indiana.
28-30 December 1978	American Philosophical Society, Eastern Division Meeting, Atlanta Hilton, Atlanta, Georgia.
11-15 January 1979	American Mathematical Society, Annual Meeting, Milwaukee, Wisconsin.
22-29 August 1979	Sixth International Congress of Logic, Methodology and Philosophy of Science, International Union of History and Philosophy of Science, Hanover, GERMANY.

IV. CONFERENCE REPORT: Ethical Guidelines for Research and Clinical Perspectives on Human Sexuality

Robert C. Kolodny, M.D.
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Newly developing fields of science are often characterized by enthusiastic attention to technology at the relative expense of consideration of fundamental issues of ethics and values. Professional interest in human sexuality from both research and clinical perspectives is no exception; it has undergone an unusually rapid growth in a short span of time, creating a situation in which workers from widely divergent disciplines must contend with similar problems in the absence of a cohesive set of common principles. In addition, members of the general public frequently possess insufficient information to identify unqualified, irresponsible, or incompetent persons working (or claiming to work) as sex researchers or sex therapists. In an effort to remedy this situation, the Reproductive Biology Research Foundation, under the direction of William H. Masters and Virginia E. Johnson, has sponsored the development of a set of ethics guidelines for sex therapists, sex counselors, and sex researchers. This project began in 1974 with the formulation of plans for a conference held in January 1976, which aimed to identify the major ethical issues in sex research and therapy.[1] Following that forum, a fourteen-member task force was organized to prepare background papers and draft a set of ethics guidelines to be considered at a subsequent meeting,[2] and the resulting Ethics Congress was held in St. Louis, Missouri, on January 25-27, 1978, with fifty participants joining in discussions of the draft guidelines.[3]

In her opening remarks, Virginia E. Johnson acknowledged the new social climate toward awareness and acceptance of human sexuality, and underscored several themes that reappeared throughout the meeting: (1) the need for sensitivity to the values and preferences of the individual; (2) the need for historical and cross-cultural perspectives in such work; and (3) the problems inherent in the professionalization of a field.

The first day of the congress was spent in plenary session with presentation of summaries of background papers, followed by panel discussions in each of the following areas: informed consent in sex research and sex therapy; privacy and confidentiality in sex research and sex therapy; the ethics of sex research involving children and the mentally retarded; value imperialism and exploitation in sex therapy; and accreditation and training in sex therapy. The overall focus of this series of sessions was on broad theoretical issues; disagreement arose principally when the perceived territorial imperative of a participant seemed to be endangered, as when a social psychologist objected to limits on research involving deception.

A panel discussion of informed consent in sex research and sex therapy produced some heated disagreement. Charles Fried (Harvard University Law School) observed that the same respect for individual liberty implicit in permitting people to pursue different sexual life-styles or in allowing people to study

sexual behavior or sexual function mandates careful attention to protecting individual liberty via mechanisms of informed consent. He added, "it is... remarkable and heartening that the guidelines... take this principle of informed consent more seriously than... any document which I have seen;... because if there is one group of professionals that ought to take the principle of liberty seriously, it is surely you, because that is the lifeline on which you depend." In contrast, John Money (Johns Hopkins University Medical School) voiced concern about unwarranted emphasis on informed consent, arguing that such requirements have resulted in loss of outcome data regarding treatment modalities, have interfered with obtaining truly random samples for research purposes, and have hampered efforts at innovative treatment or research. Money contended, "There are times when the scientist or the physician is so much wiser in the knowledge of his own speciality than is society or the law that in order to live up to his own moral standards, he must risk punishment...." Albert Jonsen (University of California, San Francisco) reflected on the differential needs for informed consent in therapy versus research, pointing out that disclosure of therapeutic uncertainty may have a detrimental effect on the client's development of confidence in the therapist and may therefore undermine therapeutic outcome. In this regard, Miriam Kelty (National Commission for the Protection of Human Subjects) suggested that because of rapidly changing knowledge concerning efficacy of sex therapy, clinicians as well as researchers have an obligation to promote openness and education in their disclosures.

Issues of privacy and confidentiality in sex therapy and sex research were discussed in a background paper by Richard Wasserstrom (University of California, Los Angeles). After analyzing the relation of privacy to sexual matters, Wasserstrom then developed an alternative theory that argued for the desirability of a cultural concept of privacy which has a more limited domain and a different form of socialization than that which currently exists. In the specific context of sexual behavior, Wasserstrom suggested that such a change in societal attitudes would "make individuals more secure and at ease in the world," would reduce individual vulnerability to invasions of privacy, and would improve the nature of interpersonal relationships in general by making them more open and less deceitful. Ralph Slovenko (Wayne State University) disagreed vigorously with this view, pointing out that privacy has positive values, such as in personality development and the establishment of close relationships, and called for an increase in the conscientiousness with which sex therapists and sex researchers protect client confidentiality. Robert Baum (Rensselaer Polytechnic Institute) further developed this point, emphasizing the intrinsic as well as the instrumental value of privacy. He also called attention to distinctions between public and private behavior as they pertain to research ethics, and discussed the varying implications of anonymity as a shield for individual privacy. Fritz Redlich (University of California at Los Angeles) chided the gathering by questioning the compulsion always to be ethical. He suggested that there is a place for unethical conduct for constructive purposes, as long as one then faces the consequences of being unethical; "otherwise [a great deal] of research could stop altogether and this would be a pity, or maybe a lack of pity, for future generations."

H. Tristram Engelhardt, Jr. (Kennedy Institute, Center for Bioethics) focused a detailed analysis of problems regarding values and exploitation in sex therapy around four general principles: (1) the use of disease language should be restricted to avoid stigmatization or unwittingly imposing cultural values on

persons; (2) condemnation of sexual practices as immoral should be avoided except for practices that are coercive, although a therapist who operates from a given moral stance may so inform a potential client prior to the inception of therapy; (3) therapeutic arguments on behalf of particular sexual life-styles should be in the aesthetic mode and not disguised as medical fact or moral certainty; and (4) therapists should not become involved in sexual relations with clients, although the use of sexual partner surrogates is justifiable if precautions are taken to avoid coercion or harm to either client or surrogate and to obtain the informed consent of both parties. The ensuing discussion of these principles wandered through an interplay that juxtaposed Kantian philosophy, the origins of religious versus secular values, the role of pragmatism in clinical work, and the most current and contemporary social issues - as in the statement by Richard Green (State University of New York at Stony Brook) that the American Psychiatric Association still shows evidence of cultural bias by its use of disease language to characterize the homosexual troubled by his or her orientation (but not the person whose problems have a heterosexual origin).

The first day of the congress concluded with an evening discussion of the training of sex therapists and other aspects of professionalization of the field. There was considerable disagreement among participants on specific details regarding the relative merits of licensure laws, certification, or no regulation of the field; there was general recognition of the lack of adequate funding, facilities, or personnel for training.

The second day of the congress began in plenary session with a panel discussion of the guideline draft, section by section. Following this, approximately six hours were spent in concurrent small group discussions, each focusing primarily on one section of the guidelines. These discussions served as the basis for extensive alteration and revision of the guidelines; on the final day of the meeting, summaries of each discussion group were presented and new versions of the guideline sections were read.

Participants were then asked if anyone opposed the release of the new ethics guidelines or wished to postpone their release. John Money spoke against the development or dissemination of guidelines; he argued instead for the development of an ethical case book, to facilitate the gathering of a data base from which ethical concepts might then be evolved from a broad viewpoint. He voiced the concern that detailed ethics guidelines could be used against sex researchers or sex therapists to suppress dissent or to prevent innovative work, and went on to say, "If we promulgate a set of guidelines in which we, the national representation of the authorities in the field, say 'Thou shalt not, thou shalt not,' as a list of commandments, then we have given too much ammunition to those who are out to get us." Samuel Guze (Washington University Medical School) concurred and stated his belief that there are a sufficient number of ethics codes already in existence to provide suitable guidance. Samuel Gorovitz (University of Maryland) suggested that the need to protect consumers in this newly developing field was sufficiently strong to justify the release of guidelines, and also observed that such a document would have the potential to influence the shape of subsequent regulatory guidelines or laws passed on a state-by-state basis. Robert Baum, speaking in favor of release of the guidelines, pointed out that such a document should not be viewed as final and unchanging; moreover, such openness to eventual revision had been clearly indicated by the philosophy and discussions of the meeting and specifically emphasized in the preamble to the guidelines.

At the conclusion of the three-day congress, the task force met to reappraise the guideline draft and to begin the final process of revision, resulting in another circulated draft and then the release of a final version of the guidelines in March 1978. The finished document consists of five sections: competence and integrity of sex therapists, confidentiality in sex therapy, welfare of the client, welfare of students and trainees, and welfare of the research subject.* The following selected points pertain to matters of controversy as they have been dealt with in the final version of the guidelines:

- "When sex therapy involves one or more therapists working with a client couple, whether married or not, unusual circumstances pertaining to confidentiality may arise. In such instances, the following considerations apply:

  (a) Disclosure of information that one client has requested be kept confidential from his or her partner should not be made without the express consent of the person providing the confidential information.

  (b) When only one client of a client couple provides consent to the release of confidential records or information, the sex therapist is responsible for releasing only information about the consenting client and must protect the confidentiality of all information deriving from the nonconsenting client." [Section II of the guidelines]

- "The client's benefit should be the first priority of any therapy. A sex therapist will not use his or her therapeutic relationship to further personal, religious, political, or business interests. Sex therapists should be mindful of the special problems attendant upon treating their employees, relatives, business associates, students or trainees, close friends, or their own sexual partners (such as loss of objectivity, diminished confidentiality, and the changing context of social relationships)." [Section III]

- "It is unethical for the therapist to engage in sexual activity with a client." [Section III]

- "Procedures involving nudity of either the client or the therapist or observation of client sexual activity go beyond the boundaries of established therapeutic practice and may be used only when there is good evidence that they serve the best interests of the client." [Section III]

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* Interested readers may obtain a copy of the complete guidelines by writing directly to the author [Dr. Robert C. Kolodny, Associate Director, Reproductive Biology Research Foundation, 4910 Forest Park Boulevard, St. Louis, Missouri 63108].

- "Client nudity during a physical examination by a licensed physician, nurse, or physician's assistant is not prohibited or unethical." [Section III]

- "In treatment of a couple, when neither client has requested that any matters be held confidential or kept secret from the other, but when the therapist(s) judges that there is a significant risk to discussing jointly information not known to one client, it is the responsibility of the therapist to point out this fact to the relevant person and to obtain his or her consent before disclosure of such information." [Section III]

- "When research or training is being done in the context of providing therapeutic services, a potential for conflict of interests exists. In individual circumstances, when it becomes known that the benefit to the client(s) is being compromised due to such practices, it is necessary to modify or terminate the research or training in order to maximize the objective benefit to the clients from therapy, even though the clients have consented to participation in research- or training-related therapy." [Section III]

- "Sex therapists should be aware of the personal value system that they introduce into the therapy context and should disclose these values to the client when such information is relevant to treatment. Moreover, therapists should avoid gratuitously enunciating opinions or prescribing values that reflect their personal biases rather than being responsive to the needs and well-being of the client. In this regard therapists should not condemn certain types of sexual practices, except those that are coercive or involve deceit." [Section III]

- "It is unethical for teachers or training supervisors to require, to coerce, or by fraud to induce their students or trainees to engage in sexual activity." [Section IV]

- "Persons seeking or pursuing training in the field of human sexuality may be queried about their private, personal sexual histories and attitudes, since these may materially affect the competence, integrity, judgment, and objectivity of professional performance. Such persons should be informed in advance of the relevant limits to confidentiality pertaining to this material, as described earlier in this section." [Section IV]

- "It is unethical to coerce or require students to participate as subjects in research projects. The increased risk of conflict of interest or subtle coercion should be recognized and guarded against by educators who engage in research with their own students or trainees." [Section IV]

- "Sex researchers should strive for honesty and accuracy in their dealings with research subjects. Concealment or deception may be used as a part of the research design only when alternate methodologies have been considered and found inadequate. In such cases, unless the risk is minimal and there is potential direct therapeutic benefit, subjects must be informed during the consent process that concealment or deception may be used. Explanation of the specific nature of the concealment or deception must be made as soon as possible after the subject's participation has ended." [Section V]

The guidelines have been developed for use by professional organizations, institutions, review boards and similar interested groups, and it is expected that the guidelines will be particularly useful in the drafting of regulatory or licensure legislation at the state level. At their national meeting in April 1978, the American Association of Sex Educators, Counselors, and Therapists - the largest professional organization in this field - adopted these guidelines as formal ethical principles for their more than 6000 members.

NOTES

1. The proceedings of this meeting were published in book form under the title Ethical Issues in Sex Therapy and Research, edited by William H. Masters, M.D., Virginia E. Johnson, and Robert C. Kolodny, M.D. (Boston: Little, Brown and Company, 1977).

2. The members of the task force were Robert C. Kolodny, M.D. (Chairperson), H. Tristram Engelhardt, Jr., Ph.D., M.D., Charles Fried, LL.B., Joshua S. Golden, M.D., Richard Green, M.D., Albert R. Jonsen, Ph.D., Harold I. Lief, M.D., Ruth Macklin, Ph.D., Jay Mann, Ph.D., Lorna J. Sarrel, M.S.W., Philip M. Sarrel, M.D., Raymond W. Waggoner, M.D., Sc.D., Richard Wasserstrom, LL.B., Ph.D., and Jerome F. Wilkerson, M.Div., Ph.D.

3. Proceedings of the Ethics Congress will be published in book form in 1979 by Little, Brown and Company, Boston, Massachusetts.

V. CHANGING DIMENSIONS OF POLITICAL ACTION IN SCIENCE

Papers and Commentaries Presented to the
Organization of American Historians, April 1978

INTRODUCTION

When science becomes controversial, scientists inevitably become involved in political activity, taking sides and raising questions about the institutions that control the direction and application of research. In a recent book on the history of physics, Daniel Kevles has suggested that many such questions tend to recur again and again at different periods of history. Indeed, striking parallels are apparent between current issues of concern and those that stimulated political actions by scientists following World War II. During both periods, scientists who were fully convinced of the usefulness of science and technology became disturbed about pernicious and dangerous applications. Disputes today and 30 years ago have both involved intense conflict over the types of control to be exercised on technological application, and in particular over the extent of self-regulation within science and the nature of new institutions for the direction and control of research.

The post-World War II debates, however, revolved around a technology with a demonstrated and devastating impact, while the recombinant DNA dispute concerns a scientific technique that poses hypothetical risks. Moreover, in the 1940s, research was a relatively small and exclusive activity compared to the sophisticated scientific enterprise of the 1970s. Furthermore, and perhaps more important, the recombinant DNA dispute takes place in a context of profound historical awareness of the experience with atomic weapons. Indeed, this experience is a popular metaphor for modern activists--as a demonstration of the destructive potential of science, an image of what must be avoided at all costs--and it has been critical in shaping the views and strategies of contemporary science activists.

-Dorothy Nelkin

The following papers by Alice Kimball Smith and Dorothy Nelkin compare various aspects of these two disputes, especially the political role of the scientists active in the discussion of policy questions during the two periods. Smith and Nelkin ask: 1) What are the major issues of concern to scientific activists? 2) Who becomes involved in policy disputes? 3) What strategies of political fluence do they use? and 4) How does the larger scientific community respond to scientific activism? Smith addresses these questions in the context of events following World War II. Nelkin examines the contemporary debate over recombinant DNA and other disputes involving scientific activists, asking the same questions and drawing contrasts between the rDNA dispute and the Atomic Scientists Movement. In subsequent commentaries, Daniel Kevles and Peter Buck not only discuss the Smith and Nelkin papers but add further interesting observations on the changing attitudes toward political action by scientists.

A. SCIENTISTS AND THE PUBLIC INTEREST - 1945-46

Alice Kimball Smith

The initial impetus to the political action by scientists that followed World War II came from within the Manhattan Project: from Los Alamos and from two laboratories of the Metallurgical Project--the Clinton Labs at Oak Ridge and the Met Lab at the University of Chicago.[1] Agitation against possible use of the bomb on Japan had developed in the Chicago Met Lab early in 1945, sparked by concern about an apparent lack of official planning for the future impact of atomic energy. At that time, Chicago scientists began a series of representations to the War Department which focused first upon peacetime applications and their regulation, and then on international implications, and culminated on 11 June 1945 in the Franck report recommendations urging a demonstration of the bomb prior to military use.[2]

The Issues

The vast majority of Manhattan Project employees, however, were not aware of this wartime agitation, and thus the postwar scientists' movement began not as an internal controversy throughout the project but rather as a spontaneous reaction to Hiroshima and Nagasaki. This reaction largely took the form of widespread resolve by scientists to inform themselves, the policy makers, and the American public of the facts and implications of atomic energy so that effective international controls might be established. In due course, this resolve focused on four issues: 1) the way the bomb had been used on Japan; 2) the right of scientists to make public statements on policy questions; 3) whether civilians or the military should control domestic programs; and 4) the freedom of scientific research. Secrecy was a recurring theme in these discussions, but to most activists there was a single overriding issue: how to avoid an international atomic arms race. As rough notes from a June 1945 Chicago "rap" session put it: "Clear that if no agreement, are sunk." Atomic bombing of Japan, as the Franck report had foreseen, had created an atmosphere of distrust that made international cooperation more difficult. Many of the scientists believed that continued secrecy in science would prevent dissemination of information they considered to be prerequisite to international control, and that continued military supervision of U.S. nuclear programs would further exacerbate the suspicion engendered by American use of the atomic bomb.

Strategies and Tactics

Because the scientists were learning the techniques of a new trade and because, even after the war ended, they lacked reliable information about official intentions, the tactics they employed to deal with these corollary issues were improvised and empirical. When the war ended, scientists at virtually every Project site set up organizations and prepared statements of purpose emphasizing education directed to the support of international control. Education remained

the principal element of the scientists' strategy, implemented largely by local association contacts with press and radio, the lecture platform, conferences for laymen, and publication of books and articles. The most enduring monument to this educational impulse is, of course, the Bulletin of the Atomic Scientists, which began as the newsletter of the Chicago association.

Overall strategy soon included centralized organization. Circulation of mimeographed material from Chicago, Oak Ridge and Los Alamos stimulated the rapid proliferation of local groups at research centers outside the Project. By early October, embryonic organizations were coalescing almost overnight to protest the War Department-sponsored May-Johnson bill, which provided for a domestic atomic energy commission with military members and drastic security provisions that seemed to threaten freedom of research. As the scientists converged on Washington to stop the May-Johnson bill, they soon recognized the need for coordination of effort; and so, in November 1945, the Manhattan Project site groups formed the Federation of Atomic Scientists (shortly thereafter transformed into the more broadly based Federation of American Scientists). At first, the Washington office was manned by volunteers on a rotation basis; later, the small full-time staff often found it easier to influence congressmen than to reconcile the conflicting views of what became 18 local associations and approximately 3000 members.

Weeks before the national organization was formed, the first critical issue had arisen: namely, the right of Project scientists to make public statements on atomic policy. The bombings of Japan and (three days later) the publication of the unexpectedly explicit Smyth report[3] lifted much of the burden of secrecy under which Manhattan Project employees had labored; but the Army was still in charge, and Project scientists were told to refrain from public discussion pending announcement of official policy. This silencing of those who considered themselves best qualified to give accurate information infuriated many scientists who had philosophically accepted wartime restrictions. The site groups preparing careful programs of atomic education were caught off guard, but the tactics adopted were no less effective for being spontaneous and uncoordinated. On September 1, the director of Chicago's new Institute for Nuclear Studies made headlines with his off-the-cuff comment that if wartime secrecy continued he and many of his colleagues would abandon physics and study butterfly wings. Less prominent physicists, chemists, and biologists used long deferred vacations for exploratory interviews with columnists, broadcasters, and public figures in New York and with key legislators in Washington, distributing statements and seeking names of persons who might publicize their views. The Association of Oak Ridge Scientists wrote to the War Department demanding explicit recognition of their rights as citizens.

The frustration of these scientists mounted during September as the press filled with loose talk about the "secret" of the bomb and possible "defenses" against it. The ban on public discussion became increasingly irksome as rumors circulated about the nature of the bill being prepared in the War Department, and Project scientists began to doubt whether their views were effectively represented by the seven prominent individuals who were part of the government's advisory apparatus.[4]

Truman's atomic energy message to Congress on October 3 and the introduction of the May-Johnson bill on October 4 were immediately seen as signals that quiet sleuthing in Washington and seemingly inadvertent leaks to the press could be replaced by overt action. By October 15, a group of hitherto unknown young scientists, who did not even know each other, had gathered in Washington. Working out of Leo Szilard's hotel room, they quickly found allies within the Administration. They made friends with Congressional aides, and recruited prominent scientists for a dinner with important senators. Learning that House committee hearings on the May-Johnson bill had closed after one day, the fledgling lobbyists promoted a deluge of telegrams sufficient to force the hearings to reopen and receive testimony from opponents of the legislation. With the connivance of sympathetic legislative aides, another Senate committee hearing on science foundation bills became a forum for discussion of atomic energy.

A Drew Pearson column had already drawn attention to earlier attempts to muzzle scientists when site representatives called a press conference at which chemist Harold Urey blasted the May-Johnson bill as a signal that "we are essentially an aggressor nation." Raymond Swing's nationwide broadcast on Friday, October 19, was devoted to what he called "science week in Washington." The scientists, said Swing, are "as impressive a group of men as ever came to modern Washington.... quiet, modest, lucid and impellingly convincing."[5] The actors in the drama were as surprised as anyone; Newsweek later dubbed them "The Reluctant Lobby."[6]

These tactics played no small part in shelving the May-Johnson bill. Beginning in December 1945, similar ones were employed on behalf of the McMahon bill, which provided for a civilian atomic energy commission and more adequately represented the views of the scientists and like-minded officials who had helped to frame it. The cause of civilian control was carried to the public by the stable of lecturers maintained by each local association. Since more experienced propagandists advised working through existing organizations, in November, the Federation of Atomic Scientists (in collaboration with forty-nine national civic, labor, and religious associations) established the National Committee on Atomic Information, which distributed to millions of affiliated members the facts and arguments channeled through the Federation. When amendments twice threatened to cripple the McMahon bill, an ad hoc committee of laymen helped the Federation devise lobbying procedures, including a massive telegraph campaign by specially recruited citizens' groups. In July 1946, the bill passed and, the following January a civilian atomic energy commission took over from the War Department.

The campaign to prevent military domination of atomic developments was closely related to the issue of freedom of science because international control would require unrestricted exchange of information; however, the scientists had found that it was not easy to agree on priorities. In mid-November 1945, when representatives of fourteen local associations had met to formulate the aims of the enlarged Federation of American Scientists, those who wanted to include only achievement of world peace through international control were vigorously opposed by a delegate from a non-Manhattan Project group in Cambridge. "If your group wants to work primarily on world peace" he said, "that is fine;... but if our group is particularly interested in freedom of speech and science and seeing to it that...[when] we get a world government...scientists are not all working behind locked doors, I think we ought to be permitted to work on that."[7] In the end,

study and dissemination of information were accepted as the basic aim and local associations were left free to determine their own emphases.

How difficult it would be to separate issues soon became clear. As the campaign for the McMahon bill began, some Manhattan Project scientists broke an earlier vow of restraint and spoke sharply about the excesses and stupidities of wartime Army security regulations. What the press described as a feud between scientists and the military evoked counter charges that scientists were merely trying to preserve their traditional independence. Scientists responded that values important to science--respect for truth, individual freedom and dignity, cooperation--were basic human values and that they had emphasized freedom of science because it was the particular freedom they were qualified to explain and defend.

Two other points were raised at the November 1945 meeting--establishment of a national science foundation and the peaceful uses of atomic energy--but they aroused little interest among the activists. The big machines and team research to which scientists had become accustomed during the war would obviously require money, but for the time being universities were generous to science. Everyone agreed that a science foundation was desirable, and the Federation office regularly exhorted members to read and discuss pending legislation; but debate over who should administer the foundation delayed passage of a bill until 1950 without arousing a hint of the fervor expended on the control of atomic programs.[8]

Lack of attention to the peaceful applications of the atom is even more curious because many scientists had invoked "ultimate value to mankind" as justification for participation in the Manhattan Project. Non-military uses had been explored in the Met Lab's 1944 Jeffries report on the future of nucleonics,[9] but after the war, preoccupation with control and uncertainty about the continued existence of the big laboratories discouraged substantial progress in these areas until the establishment of the Atomic Energy Commission in 1947.

The legislative campaign diverted much time and effort from the principal objective--international control. But for a time, interest was keen, not to say passionate, and volunteers were plentiful. In the autumn of 1945, concerned legislators were persuaded to sponsor resolutions favoring international control and to press for early talks with allies about sponsorship of the United Nations Atomic Energy Commission (UNAEC). To have information available when and where it was needed, the scientists organized a series of technical reports, some secret, some unclassified, by qualified people at the various laboratories. These reports were eventually used by the State Department committee which began preparing a U.S. international control proposal early in 1946. Later, American scientists served as consultants to the U.S. delegation to the UNAEC and as members of its subcommittee on technical aspects of control. This subcommittee's conclusion in September 1946, that control was technically feasible, represented the only substantial agreement in the protracted negotiations and was hailed as evidence that the international community of science was indeed a force for peace. Meanwhile, the impact of atomic energy on international relations had been eloquently explained to audiences, large and small, across the country in the hope (never to be realized) that an international control treaty might some day need voter support.

Who Was Involved

The scientists' movement involved individuals who varied widely both in background and in the nature of their commitment. As they had in nuclear science, the refugee scientists, such as James Franck and Leo Szilard, played an important role in stimulating others to think and act. Senior American scientists, like their European counterparts, had reached adulthood in the era of cynical revisionist studies of the origins of the First World War and the role of the "merchants of death" and therefore tended to be strongly pacifist in outlook, at least until the late 1930s. But political activism was far from the norm for American scientists; only two exceptions seem obvious among Americans prominent in framing postwar atomic policy--Oppenheimer had supported left-wing causes in California, and Arthur Compton had joined the American offshoot of the British Association of Scientific Workers when it was formed in 1938.[10]

As indicated above, only at the Chicago lab had scientists seriously questioned the necessity of using the bomb in Japan. Some of this concern filtered through strict security to the related research group at Oak Ridge, although Los Alamos was effectively insulated from the ferment in Chicago by the extreme pressure of work and even tighter security: a wartime meeting on "the impact of the gadget" had no sequel when Oppenheimer decreed such discussion premature. However, after the Alamorgordo test, suddenly everyone talked, chiefly about Niels Bohr's comforting view--of which Oppenheimer was the most eloquent exponent--that atomic energy would force a revolution in international relations.

After the war, when scientists began to make their first outside contacts, they were immediately asked how representative were their views. At Clinton Labs, Association members claimed that 96% of the civilian physicists, chemists, engineers, and biologists had signed a hastily-assembled 14 September 1945 statement on atomic energy. At a Senate hearing, a young physicist who was asked about Los Alamos involvement replied, "Before the bomb about fifty; afterward everyone." The Chicago Met Lab's claim of 95% support was cautiously revised to "over ninety percent," but, said a young activist, "that five or ten per cent included people we respected, and that always bothered us."[11]

The movement reflected the interdisciplinary make-up of the Manhattan Project, which included physicists, chemists, metallurgists, biologists, physicians, and engineers. Several women were extremely active in the movement, but they represented a small minority in science. The movement was a young man's affair, largely because there were so many in the Manhattan Project, which began recruiting after many older men were committed to other war research. Also, it was easier for the young to be amateurs. Political activity provided a new and exciting challenge, a release from wartime confinement, and a therapeutic outlet for growing anxiety over the destruction they had wrought and the problems they had helped to create. Scientists close to or over forty--the old men of the Manhattan Project--helped local associations get started and continued to give their share of public lectures, but many of them soon returned to academic life with heavy responsibilities for teaching and research. It was the young scientists who formed and ran the Federation, managed educational programs, and with a few exceptions, did the lobbying.[12]

Even at its peak, the lines between those who belonged to the scientists' movement and those who did not were shadowy and shifting. Yet, upon reflection, we see that something special was happening. Deflecting public attention from internal differences gave the movement coherence and was smart tactically, but disagreement and resolution were implicit in the research experience of most scientists at this period, so that quibbling vigorously in private before presenting some kind of consensus to the public was normal behavior.

Response of the Wider Scientific Community

The same pattern was maintained by scientists outside the movement. No one launched a counter-federation. Reasons for non-participation were usually conveyed orally and off-the-record, but chemist W. F. Giauque expressed a not uncommon viewpoint in his reply to a reproachful telegram from a former student who had deplored the lack of support from Berkeley for the Federation's November 1945 international control resolution:

> "... The group here is not actually 'aloof' in this matter, but we do not believe that there is any use in trying to get nations to renunciate or prohibit the use of instruments of war. This would seem to be particularly true of the atomic bomb, which could apparently be assembled on short notice by any unscrupulous group which acquired sufficient power to appropriate fissionable material which must be on hand in large amounts in many places if various nations are to make peacetime use of it.
>
> We do not believe that anyone has seen any real answer to the atomic bomb problem and some of us are concerned lest too much insistence on specific non-scientific objectives by scientific groups will weaken their position in the matter."[13]

Some scientists went further and argued, although not often publicly, that the U.S. should try to maintain a monopoly of nuclear technology. At the other extreme, members of the greatly overshadowed Association of Scientific Workers criticized the Federation for neglecting the broader implications of science. And scientists employed in federal agencies charged neglect of national science policy and the science foundation bills.

Just as those within the movement tried to resolve their differences in private, so for a time disagreements about policy failed to break the ties that united the larger community of science--particularly the conviction that knowledge was good for mankind and the faith that scientifically ascertained truth would ultimately lead to right solutions. Hence those who disapproved were not ready to undermine the authority of science through organized opposition.

Overt opposition was unnecessary, however, for debilitating forces were soon at work within the movement. By early 1947, the rank and file were sending ominous signals by devoting increasingly less time to Federation business; skepticism about the feasibility of international control was privately expressed when two of its most articulate advocates publicly defected, Edward Teller to world government and Harold Urey to Atlantic Union. In the absence of international

control, even a civilian domestic commission was powerless to withstand the pressures of the atomic arms race so accurately predicted by the Franck report. Failure bred apathy; the pull of the laboratory was strong. By 1949 only about 10% of the Federation's thousand members were active.

This diminished support still did not represent cleavage. When fracture of the scientific community did occur it came about more subtly--the activists would have said more insidiously--through the gradual incorporation of advocates of nuclear strength in the AEC's administration and advisory groups. This rift was partially revealed in late 1949 in the hydrogen bomb controversy, but it was clearly apparent during the 1956 test ban hearings when Edward Teller and Hans Bethe disagreed publicly not only on policy but on what each claimed to be scientific fact. On occasion, the scientific community did continue to present a united front, most notably during the 1954 hearings on the withdrawal of Oppenheimer's clearance and at the 1955 Atoms for Peace Conference in Geneva.

Reaction of the Lay Community

Every Project scientist undoubtedly had one friend or relative who asked, even in the first euphoria of victory, "Why did you work on the damn thing?" But negative reactions were often lost in the chorus of "Thank God for the bomb; my son (or husband or brother) was in the Pacific." The scientists' decision to go public forestalled much criticism, and those who realized at once what Hiroshima and Nagasaki portended--Norman Cousins was among the first with his article, "Modern Man is Obsolete"[14]--recognized the scientists as their best allies. Much help came from middle-aged "movers and shakers," veterans of liberal causes of the 1930s. Many were one-time pacifists, reluctantly co-opted for war against dictatorship, and this made them easy targets for the new message of internationalism. They were experienced in the art of mobilizing support, but they were reformers, not protesters as we understand the term today.

To politicians on Capitol Hill who were not already committed to a cold war posture, the earnestness of the young scientists was appealing and their amateurism non-threatening. A few scientists developed real political acumen, but even those laymen who attributed to them major credit for passage of the McMahon bill believed that most of the scientists failed to grasp the nuances of political relationships or to understand the appropriate function of military departments in a democratic state.

To the wider public, the bomb was a potent demonstration of the impact of a particular technology. The thousands who heard the scientists' public lectures and read their literature got the message again and again that this technology must be controlled.

Institutional Heritage

Discouraged by the impotence of the Federation of American Scientists, those most deeply concerned with the impact of science opened other channels--the Bulletin of the Atomic Scientists, the Pugwash Conferences on Science and

World Affairs, the Society for Social Responsibility in Science, the Union of Concerned Scientists, the AAAS commitment to social responsibility, and caucuses within professional societies. Since the early 1970s, a revived FAS, with twice its peak postwar membership, has addressed a broad spectrum of science policy questions. Underlying this institutional heritage of the postwar movement is its true legacy: that it is now respectable to acknowledge responsibility for research and its applications and that the public has come to expect this of scientists, even if they do not individually expect it of themselves.

One assumption that scientists of the earlier period were not yet prepared to challenge, except perhaps in moments of private introspection, was the rightness of pursuing the basic knowledge from which the technology of the bomb had sprung. That challenge was left to successors, whose attitudes are discussed in the paper by Dorothy Nelkin.

NOTES

1. For a fuller discussion of topics covered in this paper, see Alice Kimball Smith, A Peril and a Hope: The Scientists' Movement in America, 1945-1947 (Chicago: University of Chicago Press, 1965); revised paperback edition (Cambridge, Massachusetts: M.I.T. Press, 1971). All page references are from the revised edition. The book, which contains complete references to sources, is based upon interviews with participants and extensive examination of the files of the Federation of American Scientists and related organizations. These papers are now in Special Collections, The Joseph Regenstein Library, University of Chicago.

2. The version of the Franck report printed in Appendix B of A. K. Smith, op. cit. (1971), differs in phraseology, although not significantly in substance, from the copy submitted to the Secretary of War. This document, now in the public domain, is in the National Archives and Records Service of the General Services Administration, Washington, D.C.

3. Henry deWolfe Smyth, Atomic Energy for Military Purposes (Princeton, New Jersey: Princeton University Press, 1945).

4. For related background events see Richard G. Hewlett and Oscar E. Anderson, Jr., The New World, 1939/1946 (University Park, Pennsylvania: Pennsylvania State University Press, 1962). As official historians of the U.S. Atomic Energy Commission, the authors had access to classified materials.

5. The events summarized in this paragraph are recounted in A. K. Smith, op. cit. (1971), pp. 144-171.

6. "The Reluctant Lobby," Newsweek XXVI (December 3, 1945), p. 42.

7. A. K. Smith, op. cit. (1971), p. 233.

8. For the history of this legislation, see Daniel J. Kevles, "The National Science Foundation and the Debate over Postwar Research Policy, 1942-1945," Isis 68 (1977), pp. 5-26.

9. "Prospectus on Nucleonics" (November 18, 1944). Portions of this unpublished report are printed in Appendix A, A. K. Smith, op. cit. (1971).

10. The founders of the British Scientific Workers had been concerned with the broad social implications of science, but the Depression had directed attention to conditions of laboratory employment. When former members attempted to renew discussion of salaries and working hours at Oak Ridge and Los Alamos, they quickly realized that they faced far more serious problems and became leaders in the postwar site organizations.

11. See A. K. Smith, op. cit. (1971), pp. 87, 108-109, 156.

12. Older activists saw their effectiveness in other terms. Leo Szilard remained a gadfly and instigator of new schemes. Oppenheimer and Arthur Compton were frequent government advisors, a role which was gradually institutionalized. Rabinowitch and others in the Chicago orbit developed the Bulletin of the Atomic Scientists to document the new age. Once the civilian control bill had passed and UN negotiations were under way, local groups tended to concentrate on a single cause: the Cornell association, for example, on AEC clearance procedures and Berkeley on supplying technical and political reading matter to scientists abroad.

13. W. F. Giauque to Charles D. Coryell (November 10, 1945), A. K. Smith, op. cit., (1971), pp. 212-213.

14. Norman Cousins, "Modern Man is Obsolete," Saturday Review of Literature (August 18, 1945). An expanded version of this editorial was published by Viking Press in October 1945.

B. SCIENTISTS IN AN ADVERSARY CULTURE: THE 1970s

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What are the Concerns of Scientific Activists?

Recent conflicts over biomedical research have focused on two concerns: the present and potential impacts of science, particularly the potential health and environmental hazards inherent in research procedures, and the process by which science is directed and controlled.[1] The dispute about recombinant DNA was initiated when a group of scientists voiced their fears that this research technique might inadvertently produce new and dangerous forms of infectious microorganisms for which people have no resistance and medical science no cure. These concerns were rapidly taken up by activists within the scientific community who argued for the necessity of limiting the research and for imposing external controls.[2]

The issue of biohazards is complicated by technical uncertainty. Understanding of the process by which new pathogens are produced is limited; there is no definitive means to assess potential risks. Such uncertainty leaves open large areas of disagreement, reflected in bitter disputes among scientists about the nature and extent of risk and the precautions necessary to contain them. Critics have pointed out that, even under the most rigorous procedures, the record of containment of biohazards is not flawless and that a possible accident, however remote, could be catastrophic. As research has increasingly confirmed the safety of the procedures and the adequacy of NIH guidelines to minimize risk, the criticism has not abated, for a significant aspect of the concern of scientific activists is the potential application of recombinant DNA research towards pernicious ends. The technique of isolating and recombining fragments of DNA has removed some of the obstacles to genetic engineering by allowing scientists to transfer hereditary characteristics. Critics, fearing the potential misuse of such research, opened the National Academy of Sciences Forum on recombinant DNA by singing "we shall not be cloned." Biology, they claim, is a "social weapon" that can be used to justify and perpetuate existing inequalities or to develop means of social control.[3]

The recombinant DNA controversy has brought together many of the participants from other recent scientific controversies such as the genetics-IQ controversy and the XYY dispute. These activists worry about any research which relates genetically-mediated characteristics to human behavior. Convinced that knowledge cannot be disentangled from its impact and its use, today's critics, in contrast to the activists in the 1940s, argue that questions must be raised about basic research as well as its application. And they cite the history of the use of atomic weapons as proof of their contention that once knowledge exists it is very likely to be utilized.[4] This history remains a source of the emotional character of the scientists' movement in the early 1970s.

These concerns inevitably generate questions about the process of decision-making in science: Who should control scientific research and direct its priorities? What are the appropriate institutions for control? To what extent should laymen be involved? The most radical activists in the scientists' movement are critical of the self-regulating mechanisms of science, and also far more cynical about established political institutions than their ancestors in the 1940s. They argue that expertise is not a sufficient basis for authority and seek greater lay involvement in decisions about science, which they define in political terms. These critics seek basic systemic changes in the traditional organization and control of science itself, a demand that stands in sharp contrast to the efforts during the post-war period to enhance the autonomy and self-regulation of science.

Who Becomes Involved?

Through the years the scientific movement has involved a spectrum of activists with quite different orientations. Today, they include members of ideological groups, such as Science for the People, who are highly critical of established science and technology and who seek basic social and political change. They also include spokespersons for environmental organizations, essentially professional scientific activists who regularly take positions on controversial national science and technology policy issues such as nuclear power or recombinant DNA.[5] Finally, we have seen the development of public interest scientists, who take a more pragmatic and conservative approach, emphasizing the need to provide information and technical assistance to citizen groups on specific issues, especially those relating to arms control. With their faith in the efficacy of education and public information, these public interest scientists are, ideologically, direct descendants of the post-war activists, even to the point of using the Bulletin of the Atomic Scientists and the Federation of American Scientists to document their position. However, while many scientists in the 1940s went to the public arena reluctantly, as if it violated the norms of science, today's activists seek public involvement with verve and enthusiasm.

Perhaps the most striking aspect of the recent scientists' movement is the existence of a cadre of professional activists. Often the same people speak out on any potentially controversial issue, regardless of its disciplinary emphasis--military research in universities, nuclear waste disposal, genetics research, recombinant DNA, or sociobiology. While these professional activists are few in number they can, however, count on the support of a much larger group of young scientists who were politicized during the Vietnam War and sensitized during the 1960s to the potential misuse of scientific research.

The development of a professional cadre of scientific activism is also evident in the less radical public interest science movement. Efforts have been made in many arenas to create organizations that would provide paid careers in new public advisory roles and to maintain long-term informational and educational services.[6] These efforts to institutionalize an active scientists' movement have already created a nascent infrastructure. Networks of scientists are available who can be called upon to advise citizen groups on particular issues, and organizations of politically active scientists have proliferated. Their names indicate the range of their interests: The Center for Science in the Public Interest, the

Nader Clearing House for Professional Responsibility, Science for the People, Scientific Workers for Social Action, Aerospaced, Computer Professionals for Peace, and the National Coalition for Responsible Genetic Research.

Finally, a distinguishing characteristic of the contemporary scientists' movement is that it involves not only the elder statespersons of science and those with appropriate expertise, but anyone with the inclination or even ideology to deal with a specific problem. Many scientists, however, fear that it may also bring about increased politicization of science and, with it, greater external controls.

Strategies of Political Influence

A striking feature of the new scientific activism is the public nature of its activities and the willingness of activists to engage in and, indeed, to abet political controversy. Disputes among scientists are normally resolved within the scientific community using well-established provisions of collegial review. However, recently, scientists appear willing to air grievances in a political forum--through the mass media, litigation, or appeals to citizens groups or political representatives. For example, Boston and Cambridge scientists who opposed Harvard University's plans to build a recombinant DNA laboratory presented their concerns to Mayor Alfred Vellucci, well known for his willingness to attack the academic community. They ran public workshops and talked to technicians and spoke to local community groups.

Normally, scientists seek legitimacy for their views through the support of their professional colleagues; those most active in the current controversies, however, tend to seek a broader constituency, appealing to groups external to science to support and implement their ideas. To be sure, the 1940s activists also sought a public constituency when they lobbied in Congress, contacted the press, lectured, and wrote articles in the Bulletin to educate the public about the implications of atomic energy and the need for effective international control. However, citizen participation is sought today for a different reason--as a means to increase the political accountability of science. While activists in the 1940s fought against political control over research, their recent counterparts--by calling public attention to conflicts of interest within the scientific community--seek to increase political control. Such actions have polarized the scientific community, as less radical scientists seek to maintain intact the principles of autonomy and self-regulation that were fought for by activists nearly 30 years ago.

What Is the Response of the Wider Scientific Community?

Scientists who have attempted to extend their skills to the political arena have always encountered considerable ambivalence among their colleagues. Haberer, writing 10 years ago about the political relations of science, observed that "Politics has been considered an alien element, essentially destructive of scientific endeavor."[7] Similarly, prior to World War II, Bernal observed that

"any attempt on the part of the scientist to think for himself outside his own field exposes him to severe sanctions... It is argued that in the interests of science it would be far better for him not to do so."[8]

While these norms have begun to change, many of the demands and strategies of the scientists' movement over the last few years are especially menacing in light of external pressures that threaten to diminish the autonomy of science. The public disagreements that arise among scientists as they engage in political activities could open scientific research itself to political attack, and bring further outside pressure from skeptical legislators such as Senator William Proxmire and Representative Robert Bauman.[9] These fears are exacerbated by discussion of the application of "sunshine laws" to research proposals, and of lay participation in review boards. The politicization implied by the activists' demands for accountability, it is feared, could result in external controls exercised by groups that poorly understand the nature and imperatives of scientific research and implemented only at great cost to the progress of science. Thus, as activists seek public support for their ideas about control of research, the majority of scientists tend to fall back on the apolitical nature of research and argue for scientific autonomy.

In response to proposals of legislation imposing measures of external accountability, scientists often argue that research must proceed under the old rules, relying on the judgment of peers to evaluate questions of risk as well as the quality of the research. In a statement to the National Academy of Sciences, Philip Handler, expressed the prevalent attitude:

> I view with great alarm the prospect of any law that would authorize government officials to determine what subject matter it is permissible to investigate as well as the manner in which such research is to be conducted. It would be a first step along a dimly perceived trail... As a minimum, one can foresee constraints that will swathe research with bureaucratic complexities, will increase costs, will extend the time required for the gathering of information and generally frustrate a career in research. If pursued yet farther, science could be shattered.[10]

Any external regulation tends to be resented as an intrusion. For example, when the EPA sought opinions from eminent molecular biologists about the risks of recombinant DNA research, most replied there was no reason for "interference" which would "border on the ridiculous." "The whole question of regulation and monitoring is abhorrent especially when done by a government agency and not by scientists."[11] Even those biologists who had initially called public attention to the possible hazards involved in recombinant DNA research withdrew when the issue became a source of divisiveness within the scientific community. In an extreme but not unrepresentative statement, James Watson dismissed those scientists who questioned recombinant DNA research, as "kooks, sl[illegible], and incompetents."[12] Others referred to "irresponsible ideologues" or "anti-intellectuals." The demands for constraints in certain areas of research were labelled "Lysenkoism," or "McCarthyism." The appeal for greater public involvement, it was feared, would bring unrealistic demands for 100% risk-free research and inevitable outside controls.

In much the same language that was used in the 1940s, the established scientific community has argued that science is an international activity; if we do not use recombinant DNA techniques, scientists in other countries will. But this logic is used today to argue against imposing regional or national constraints: given the difficulties of international control, a policy to constrain American science would give Europeans a leading edge in the competition for research advantage. Clearly, faith in the possibility of effective international monitoring and control has totally eroded in the present political climate of science.

Comparisons and Conclusions

In recent years political activity among scientists has intensified as each new round of questions is raised about the value of science, the limits of inquiry, or how to direct science and technology toward socially useful ends. A long period of consensus and, indeed, of unquestioning enthusiasm about science and technology has ended. Again and again, evidence of profound skepticism has appeared in environmental disputes, in the nuclear debate, in discussions on research funding, and in conflicts over many areas of biomedical research. More recently, intense concerns about the social impact of science and the potentially pernicious applications of scientific findings have been expressed in disputes over XYY research, the genetics-IQ controversy, protest against the testing of psychotropic drugs on disturbed children, and the conflict over fetal research. All these concerns have thus come to focus during the dispute over recombinant DNA research--in many ways a symbol of changing attitudes toward science, an "atomic bomb" of the 1970s.

The activism of the 1940s left an important legacy--the concept that scientists must acknowledge responsibility for the use of their research. But the implications of this responsibility have extended far beyond the terms discussed during the 1940s. The issue today still hinges upon the question of "who should control," but the arms race and the uncontrolled development of nuclear power have shaped the views of the present movement. Today's activists insist on controlling science itself, not merely its applications. They have posed a new question: should certain research be done at all? Moreover, today's activists do not trust the ability of the scientific community to regulate itself; they question the conflict of interests within competitive areas of science and propose measures for external control. Thus, the current movement is marked by far greater awareness of the politics of science and its role as an important political resource.

For both pragmatic and ideological reasons, today's activists bring their concerns to the public arena. Like the scientists' movement in the 1940s, there is considerable faith in education and in the importance of disseminating information to the public, but in the context of "sunshine laws," information has assumed much more political salience. However, in the present climate of skepticism about science and technology, activists find that, to influence science policy, they must also mobilize citizen support.

Much as in the 1940s, many scientists are either apathetic or alarmed by the political activities of their younger colleagues, whom they perceive as fragmenting the scientific community and encouraging outside controls. Indeed, both fragmentation and the threat of external controls are increasingly important realities.

In view of the changing nature of science, its widespread social implications, and the diverse policy roles assumed by so many scientists in contemporary society (as research managers, advisers, and consultants as well as academic researchers), political activism among younger scientists is likely only to increase. Disputes within the scientific community will continue to highlight the political and value dimensions of many issues that were once assumed to be solely in the technical domain. Just as the scientists' movement in the 1940s helped to shape the relationship between science and society for several decades, so the activism of the 1970s is of considerable importance in renegotiating this relationship in terms more appropriate to the present social context.

NOTES

1. See expanded discussion of these concerns in Dorothy Nelkin, "Threats and Promises, Negotiating the Control of Research," Issue on Limits to Scientific Inquiry, Daedalus (March 1978). There are several books on the controversy that dwell on these concerns; see especially June Goodfield, Playing God (New York: Random House, 1977).

2. A comprehensive review of the early recombinant DNA dispute appears in the Congressional Research Service Report for the Subcommittee on Science, Research and Technology, U.S. House of Representatives, Genetic Engineering, Human Genetics and Cell Biology (December 1966). Documents are reproduced in National Institutes of Health, Department of HEW, Recombinant DNA Research (Washington, D.C.: U.S. Government Printing Office, August 1976).

3. Ann Arbor Science for the People Editorial Collective, Biology as a Social Weapon (Minneapolis: Burgess, 1977).

4. They also use the continued conflict over nuclear energy to argue for early public debate about genetic engineering. In a letter to the NIH, the Friends of the Earth wrote: "We have been particularly struck by the small, preliminary steps being taken to deal with genetic engineering problems, with the parallels to the nuclear power controversy which of course received no public debate or scrutiny for the first twenty years of its existence. Both nuclear power and genetic engineering seem to be proceeding on the assumption that they must proceed, yet no public debate had been initiated on genetic engineering even now as the impetus grows." Letter from Lorna Salzman to Donald Fredrickson (May 17, 1976), U.S. Department of HEW, p. 542.

5. See Joel Primack and Frank Von Hippel, Advice and Dissent (New York: Basic Books, 1975).

6. This is, in part, the basis for the proposed National Science Foundation's Science for Citizens Program.

7. Joseph Haberer, Politics and the Community of Science (New York: Van Nostrand Reinhold, 1969).

8. J.D. Bernal, The Social Function of Science (London: Routledge and Kegan Paul, 1939).

9. Senator Proxmire is known for his "Golden Fleece Awards" for "frivolous" research topics. See Leigh Shaffer, "The Golden Fleece," American Psychologist (October 1977), pp. 814-823.

10. Philip Handler, Annual Report by the President, National Academy of Sciences (April 26, 1977).

11. From correspondence made available by the Environmental Protection Agency.

12. James Watson, public speech cited in Chemical and Engineering News (May 30, 1977).

C. NOTES ON THE POLITICS OF AMERICAN SCIENCE: COMMENTARY ON PAPERS BY ALICE KIMBALL SMITH AND DOROTHY NELKIN

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We have here two papers which build one upon the other. Smith's admirable treatment, a précis of her book, A Peril and a Hope, provides a reference point for Nelkin's, whose acute analyses of the contemporary movement are enriched by her historical perspective. Both papers raise important substantive issues about the politics of science; but just as Smith's paper provides a benchmark for Nelkin's, so it would seem that we need a historical frame for Smith's. Examination of certain key features of science and politics before World War II will increase our understanding of how the politics of science has changed since 1940 and how it has not.

Smith's prewar political history of the nuclear control activists needs refinement. We are led to believe that prior to World War II they were apolitical, not involved actively in political causes, and, as a result, had to learn the techniques of a new trade beginning with the fight against the May-Johnson bill. Behind such a characterization lies the assumption (in whose tacit expression Smith has considerable company) that with respect to politics scientists were notably different from other groups in the United States. True enough, scientists differed from, say, candidates for political office or lobbyists or reformers--but so did most Americans. Most people were not political activists. At the same time, most professional people, including scientists, were politically aware; they read the newspaper, discussed political issues, sometimes walked precincts, and usually voted in local, state, and national elections. It is important not to accept as paradigmatic J. Robert Oppenheimer's testimony that before 1936, when he suddenly discovered politics, he neither read a newspaper or major magazine nor owned a radio.

More important, it is misleading to permit the political history of the Los Alamos generation of physicists to exemplify the historical involvement of scientists generally in politics. Such physicists as Robert Millikan, Karl T. Compton, and other activists in the National Academy of Sciences participated in governmental affairs before World War II. While not stump speakers, they did mobilize support on various public policy issues within the scientific community; they did seek out and negotiate with elected and appointed governmental officials, and they testified before Congressional committees on legislation of importance to them and their constituency. In addition to the physicists, many geologists, especially the contemporaries and successors of John Wesley Powell, were active in the conservation movement. Powell was a political activist of the first order; in the interest of geology, the Geological Survey, or land and water reform, he could buttonhole Congressmen, line up committee chairmen, and effectively generate public support. From the late nineteenth century through the 1930s the conservation movement is filled with earth scientists who followed his good political example. Agricultural scientists lobbied successfully for agricultural experiment stations at the level of state, and then of national, politics and joined with their allies in the Department of Agriculture to enlarge the scope of federally

sponsored agricultural research through the passage of the Adams Act in 1906. Through the years up to World War II, these agricultural scientists acquired considerable political expertise in dealing with the federal Executive Branch and Congress, as well as with local governors, legislators, and farmers, to protect their professional interests and to shape public policy concerning agriculture. Chemists like Harvey Wiley did much to secure passage of the first Food and Drug Act; members of the National Bureau of Standards crusaded for honest weights and measures during the progressive era. And many biologists involved themselves in the eugenics movement after 1900 and vigorously participated in the political movement to restrict immigration to the United States.

Obviously, the involvement of scientists in politics in the mid-1940s was not as such new, but, in certain key respects, the mid-1940s movement did represent a break with the past. First, physicists were not prominent among the prewar political activists; they obviously were very prominent among the postwar group. Second, prewar political activists tended to be employed in government scientific agencies, state or federal; the postwar activists were drawn disproportionately from the academic world. Third, prewar activism tended to involve either professional self-interest or domestic social reform; the postwar activism replaced domestic reform with a concentration on foreign policy. Fourth, prewar political activity tended to be directed more at the executive than the legislative branches of government, and it was done more in camera or committee than on the stump. The scientists' movement of the mid-1940s aimed vociferously to shape public opinion and brought outright pressure to bear upon the Congress. However, it should be noted that, following the establishment of the Atomic Energy Commission and the failure of the Baruch Plan in the United Nations, the mode of political activism from, say, 1950 to 1967, reverted strongly to dealings in camera with the Executive Branch.

To turn more directly to the substance of the two papers--both are constructed in deliberately parallel fashion and are organized around the same four problems: issues, participants, strategies, and response. The parallelism of structure immediately spotlights certain key points: first, the post World War II dispute revolved around a technology in hand while the current debate centers on the hypothetical risks of a scientific technique. Second, the participants in the current dispute--the ideologues, environmental activists, and public interest scientists--are drawn from a much wider population than the scientific crusaders of the 1940s. Third, while the members of the atomic scientists' movement were dubbed the "reluctant lobby," many contemporary activists relish public controversy. And fourth, there is considerably more apprehension within the larger scientific community now than in the 1940s about the threat of political interference with science.

Although pointing to these important differences, the analytical framework of Smith and Nelkin could be made more useful by widening and deepening it. To that end, consider the politics of the science discussed here in relationship to: 1) the general self-interest of scientists; 2) the various interests of particular scientific groups; and 3) the attitudes that shape the political behavior of scientists.

Nelkin has stressed the contemporary concern of many scientists for maintaining their professional autonomy. Without in any way casting aspersions upon the sincerity of Smith's scientists--ideals and self-interest often reinforce each other--do we not find similar professional concern evident in the atomic

scientists' movement of the 1940s? At times, Smith seems to subscribe to the notion often embraced by members of the movement--that theirs was an entirely selfless enterprise. Yet Smith is too good a historian to swallow that assumption uncritically. She recognized the self-interest of her subjects in the delegate from Cambridge who, at an early meeting of the FAS, stressed that his group was concerned at least as much with freedom of science as with world government. The scientists' opposition to military control of the atomic energy program rested on more than their dissatisfaction with encouraging the rest of the world to believe that the United States wanted an arms race. It also rested on their unhappiness over the prospect of having to submit decisions concerning the course of nuclear research to the judgment of some military, rather than scientific, hierarchy and to do research under a shroud of secrecy. To the atomic scientists, such control and such secrecy were reprehensible not only in principle and not only because of their implications for foreign policy, but also because they threatened to interfere with professional activity, competition, and careers.

The self-interest that operated in the movement was made retrospectively evident by the attitudes manifest among nuclear scientists in the postwar decade. Despite the establishment of a civilian Atomic Energy Commission, secrecy was the order of the day, as it had to be, in the area of weapons technology--in short, in precisely the area pertinent to the question of international control of weapons. At the same time, while secrecy continued to prevail in some areas of basic nuclear research (probably for the most part those conducted at AEC laboratories), it was largely absent in the discipline's academic enclaves. There were complaints about the situation, to be sure, but no major ongoing outcry, not the least, one would think, because the professional self-interest of basic nuclear scientists was largely being well served.

A similar argument might be made about the 1945 concern with military control. There was no military control of basic nuclear research after World War II, because the Atomic Energy Commission was a civilian body. Although the AEC attempted, often with considerable courage, to temper the arms race, it nevertheless also acquiesced to it. Again, no significant outcry emerged from the bulk of nuclear scientists. In part, the scientists assented because they accepted the logic of the Cold War, of Russia's apparent intransigence. But, in addition, the key decisions about the government's basic nuclear research program were being made by their professional peers and, hence, their self-interest in professional autonomy was well protected.

Smith points out the interesting fact that the controversy over the National Science Foundation failed to stimulate the fervor typical of the movement for nuclear control. Of course, the NSF issue was by no means as cataclysmic in its implications as that concerning the control of nuclear energy, but the activists of the day had a certain self-interest ensuring that the Foundation be professionally autonomous and not be subject to "political" interference. Their failure to lobby more vigorously for the establishment of the Foundation was no doubt linked to the fact that money for such academic research as the NSF was to support began flowing from Washington in the Fall of 1945, via what soon came to be called the Office of Naval Research. Furthermore, even though this money was dispensed under nominal military auspices, it came without secrecy restrictions and it was dispensed substantially in accord with the judgment of professional scientists. If the nation's physical scientists were less exercised over the NSF, including the delay in its passage, than over the AEC, it was possibly because they did not feel any compelling need for the Foundation.

To turn from general considerations to the various particular interests at work, it would be helpful to know more about the political sociology of the nuclear control movement. Smith reports that the older, more established members of the scientific community lent their prestige, sometimes their voices and protests to the movement, but the more active participants, especially the Washington lobbyists, were on the whole younger people who were professionally less well established. What were their professional identities? Were they Ph.D.'s? Were they ambitious research scientists? What were their institutional interests? Answers to such questions might well help us to understand the relationship between political movements in science and the structure of the professional scientific community. For example, perhaps a young scientifically-trained person who is less interested in a serious research career may be more likely to become a political activist. Then, too, many of the younger members of the Manhattan Project went from undergraduates to de facto Ph.D.'s during the war; they had developed no institutional interests by 1945 and, hence, had no institutional interest to protect. They were, in a sense, floaters, suspended between the war and peace, between their professional training and professional careers. In transit between these two phases of their lives, they were more likely, perhaps, to take an active political role.

Possibly the decline in the movement derived in no small part from the termination of this transit phase; the floaters became professionally ensconced once the postwar institutional situation took shape, once funds were obtained, jobs established, younger people hired. Also, the increasing dependency of scientists (including the activists of 1945) upon the federal government may have well diminished their enthusiasm for protesting federal policies, at least openly. A political sociology of science would certainly need to measure the public policy positions of individuals or groups against their particular individual or institutional interests, especially with respect to government funding agencies. Certainly, the opposition to the atomic scientists' movement at Berkeley, as exemplified in the report of the chemist W. Giauque, rested on more than any inability of the Berkeley group to recognize the need for persuading nations to renounce or prohibit available weapons of war. One can respect that position, but we must also examine whether that position might not have been reinforced by E.O. Lawrence's dislike of political involvement and by the dependence of the Berkeley Radiation Laboratory upon Manhattan Project funds, including the special grant that General Leslie Groves made to complete the construction of Lawrence's 100 MEV accelerator.

Similar queries come to mind with respect to contemporary activism involving the dispute over research with recombinant DNA. Clearly, as Nelkin points out, the less radical scientists seek to maintain intact the autonomy won in the federal grant system at the end of World War II; indeed, they fear that too much agitation about the issue will inevitably politicize science. Yet if they speak for maintaining professional autonomy, why do others seem willing to jeopardize it? Nelkin suggests implicitly that it is because the activists are more radical. Perhaps they are, but perhaps, too, an analysis must be made across a dimension different from radical/liberal/conservative--the dimension of particular professional self-interest. Thus one wonders: are those who worry about professional autonomy on the whole more actively engaged in serious research than those who are not? Similarly, of those activists who are engaged in research, how many rely on recombinant DNA techniques? Perhaps the activists do not fear political interference with science, in the sense of restricting the latitude of genetic research, because such interference will not jeopardize their careers.

I think Nelkin is right that historical awareness--especially of the atomic weapons precedent--has played a significant role among the activists and their sympathizers. So also have the effects of the Vietnam War, including a general distrust of authority, of organizational aims, of institutional interests. I would add that, unlike their predecessors of the 1940s, this generation of young scientists has known only the system of federally-funded big science; its members yearn much less for the establishment and recognition of their profession. In seeking recognition from the larger society, their predecessors had left unquestioned the merits of what science might do; indeed, they had usually pointed to its possibilities as an argument for respect and funds. This generation, in contrast, having come to maturity in a professional context of astonishing national regard and affluence, is less disposed to appeal for professional recognition and is more disposed to query the premise that the advance of science is in all respects beneficent.

Furthermore, surely the employment situation for young scientists has affected their propensity for political involvement. Environmental and public interest groups seem heavily populated with scientists who have simply been unable to find jobs they consider appropriate. Many other young scientists may be employed, but their futures are uncertain in this era of restricted funding and academic cutbacks. Their situation is radically different from that of their predecessors of the 1940s. It is not surprising either that they should resort to political activism or that, even though professionally trained as scientists, they should be less concerned with the maintenance of scientific autonomy than with making more responsive to the public interest a system which, in their view, has misused them or threatens to do so.

To support and extend Nelkin's points, in key respects the contemporary movement does seem to be something new in the annals of science and politics. The proposal to interfere with the professional autonomy of science comes from within the community rather than from without. Furthermore, the object of proposals for regulation and control is not a new technology but the process of basic research itself. Finally, the scientific community itself is considerably more pluralist than it was in the 1940s, let alone before World War II, and significant groups in that pluralist constellation are willing and eager to engage in public political controversy.

The proposal to interfere with basic research is somewhat worrisome, yet I am not sure that the proposed interference is very different from traditional actions in the regulation of public health and safety. Not worrisome at all but on the contrary quite healthy, I think, is the development of a pluralist set of institutional or professional interests in science and the propensity to contest these interests in the public forum. Science as well as the public was the better for the public contest in the 1940s, and both are better for it now. We may conceivably revert to a situation characteristic of the period between 1950 and 1967, with high demand for professional research scientists and high funding for them, too; but I doubt seriously that the politics of science would revert to the characteristics of before 1940 or of 1950-1967. Time's arrow tends to proceed in one direction; it rarely goes backward, not even in the politics of science.

D. IMAGES OF THE SCIENTIFIC "COMMUNITY": COMMENTARY ON PAPERS BY ALICE KIMBALL SMITH AND DOROTHY NELKIN

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As Dorothy Nelkin astutely observes, the current recombinant DNA dispute has been affected by our previous "experience with atomic weapons." The bomb has indeed provided a crucial metaphor for the discussion of scientific knowledge and public policy--"a demonstration of the destructive potential of science," as she describes it, "an image of what must be avoided at all costs." But to call our awareness of this experience profoundly "historical" strikes me as more than somewhat misleading. I would emphasize instead how fundamentally ahistorical, if not anti-historical, our view has been, not only of atomic weapons and their political significance, or of recombinant DNA and its implications, but also of the politics of science generally. That discussions so frequently include images of "what must be avoided at all costs" suggests how far we are from regarding "conflicts of confidence" related to scientific knowledge as similar to the contingent clashes of values and disparate visions of reality of general history. Rather than showing historical awareness, our expectations about science and public policy have exhibited that "notorious" American penchant, to use J. G. A. Pocock's phrase, for seeking "to escape history and then regenerate it."[1]

In part, this situation simply reflects the rhetorical excesses built into the apocalyptic (rather than historical) language used to describe the catastrophes in store for us if we do not properly order the relationship of scientific knowledge to public policy. The remark Alice Kimball Smith has culled from a June 1945 Chicago "rap session" on the importance of avoiding an atomic arms race catches the spirit in which every major issue concerning the politics of science over the past three decades has been approached: "clear that if no agreement, are sunk." There was, of course, no agreement, and we have not yet sunk, but that has neither kept us from sighting rising water all around nor prevented those already in the lifeboats from concluding that the ship is nonetheless foundering from overloads of environmentally or spiritually contaminating sciences and technologies.

No less millenial, and no more historically grounded, are the pictures painted of the future we will inherit should we manage to bring about a proper integration of science and politics. Although Nelkin and Smith have--and quite properly, given their charge--focused on activist scientists, we should also acknowledge the flavor of the orthodox position, which James Watson's oft-quoted characterization of the participants in the rDNA debate only begins to catch. As a field of study, science policy has long appeared to be awaiting the imminent arrival of the New Jerusalem envisioned by the first social scientists. Just as Saint-Simon and Comte hoped, age-old disputes about power, justice, and the good society seem on the verge of disappearing in a world soon to be freed from the tangle of social structural constraints and ideological commitments which, historians are taught, customarily shape all forms of social choice. If the Bulletin of the Atomic Scientists has encouraged us to think of the future in terms of a clock dangerously close to striking a final, convulsive midnight, students of science policy are

apparently also ready to assure us that the countdown will at least proceed according to thoroughly rational decision-making processes employing the best scientific principles and methods.

Against this background, the papers by Smith and Nelkin are admirably deflationary. Instead of inviting us to contemplate transcendent scientific rationality as it transforms our political life, or to ponder cosmic choices between the good and evil posed by scientific and technological innovations, they direct our attention to scientists engaged in political action. And they indicate that politically active scientists may be viewed profitably from the perspectives commonly used to study other persons who practice the arts and sciences of politics. What issues capture the attention of scientific activists? Who are these scientists? What strategies do they pursue in seeking to realize their political aims? What responses do they evoke from other scientists? Although these are relatively straightforward questions, to present them as the first questions to be asked about "conflicts of confidence" surrounding scientific knowledge and public policy raises some promising possibilities.

Consider, for example, what issues have been or are of concern to activist scientists. For the most part we proceed as if it were obvious why and how certain scientific and technological developments provoke political disputes; the implication is that, in effect, controversies grow up around innovations which are intrinsically controversial. Yet even in the cases of atomic weapons and recombinant DNA, Smith and Nelkin may be interpreted as suggesting that the conflicts were in part manufactured, in the sense that for any political dispute to take shape, there must be some interested, articulate, and politically visible individuals and groups ready to press the points at issue, no matter how obvious those points may be. Perhaps it is only a small advance to see the conflicts of confidence besetting science and technology as conflicts among men and women, rather than conflicts that set people against machines or ideas, but it is an advance.

Similar considerations apply to that "long period of consensus and indeed of unquestioning enthusiasm about science and technology," on the significance of whose dissolution Nelkin rightly insists. It too was manufactured; we need to understand how and why, lest we continue to believe that consensual enthusiasm has been so normal an attitude toward science and technology that it requires no explanation; or that such consensuses have always been constructed on the same grounds and in the same terms. But the sources and structures of political consensus are irreducibly institutional and intellectual, and such matters have long proved resistant to analyses of the sort that Nelkin and Smith have undertaken. The strategy they have adopted is essentially Namierite, for they pose the crucial Namierite query, "who are these people and what do they want?" This approach not only promises no explanation of the role of ideas and institutions in shaping political behavior, but also presumes that that role will turn out to be minimal. As an approach to The Structure of Politics at the Accession of George III, this strategy may have been reasonable and sufficient. Applied to scientific knowledge and public policy in late 20th-century America, however, it unnecessarily restricts our field of vision. And, precisely because it does not encourage careful scrutiny of political ideas, it may cause us to mistake for analytic categories the contentious formulas scientists have framed to demarcate the political questions which can be legitimately asked about the scientific enterprise.

Nowhere is the problem clearer than in the casual and quite uncritical way we all speak of scientists in the aggregate as forming a "community." Both in the immediate post-war debates over atomic weapons and in the current controversy about recombinant DNA, questions about the autonomy of this aggregate have evoked passionate responses from scientists. In part this reaction is due to the symbolic value attached to the concept of community. It is, for example, difficult to visualize arguments about the autonomy of scientific unions having quite the same purchase on our political attention, or to believe that our commitments would be quite so vigorous if we saw the institutions of American science as Max Weber did. In the "external conditions" of science in the United States at the time of World War I, Weber found a distinctively "American system." The emergence of "large, capitalist, university enterprises," indistinguishable from other "state capitalist" organizations, had produced "the same condition that is found wherever capitalist enterprise comes into operation: the 'separation of the worker from his means of production.'" Far from being autonomous, the American scientists described by Weber were wholly "dependent upon the implements" put at their disposal by their employers, a development which corresponded "entirely to what happened to the artisan of the past."[2]

This characterization of American science and its institutions is, of course, political and tendentious; but so, too, are accounts built around images of scientific communities. To juxtapose the two is to see a striking paradox, the dimensions of which may be grasped by reflecting on the ease with which sociologists were able once to distinguish between traditional and modern forms of social organization by drawing sharp contrasts between Gemeinschaft and Gessellschaft, community and society. To speak of scientific communities is, in effect, to try to describe that most modern of intellectual enterprises in terms of categories devised to characterize the most traditional social relationships. Therefore, it should not be surprising that "crises of confidence" now envelop the politics of science. Our image of the community of science simultaneously reinforces our inclination to set the history of science apart from the course of history generally, leads us to see stark oppositions between scientific ideals and the realities of scientific practice, and finally, encourages the dream that a proper union of science with public policy may reinvigorate the pre-modern and anti-industrial values, symbols, and social forms identified with a lost sense of community. This is an extravagant expectation, but it is recognizably American in its antihistorical promise of both an escape from history and a regeneration of it.

NOTES

1. J. G. A. Pocock, The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition (Princeton: Princeton University Press, 1975), p. 545.

2. Max Weber, "Science as a Vocation," in From Max Weber: Essays in Sociology, trans. and ed. by H. H. Gerth and C. Wright Mills (New York: Oxford University Press, 1958), p. 131.

VI. ADDITIONS TO THE GENERAL BIBLIOGRAPHY

Akin, William E. Technology and the American Dream: The Technocrat Movement, 1900-1941. Berkeley: University of California Press, 1977.

Historical account of the American "technocrat" movement; in particular, the attempts to implement the ideas of Frederick W. Taylor and Thorstein Veblen. For a brief period during the 1930s, technocratic analysis seemed to provide an explanation for the economic crisis, but the movement was never able to muster sufficient political or social strength to have lasting influence.

Anderson, Ronald E. "Value Orientations of Computer Science Students." Communications of the ACM 21 (March 1978), pp. 219-225.

Results of a survey of undergraduates majoring in sociology and computer science, designed to "investigate the interrelationship of technological values with other values and opinions." Using the Rokeach list of terminal values and cluster analysis, the researchers identified clusters of values that align in three groups: "protestant-ethic value orientation," "technology value orientation," and "humane value orientation." The computer science students showed greater tendency to rank or differentiate among values, showed greater value consensus and possessed a more complexly organized set of value orientations. "The computer science students apparently do not accept the simple dichotomy between technology and the individual, although it seems social science students do"(225).

Baltimore, David. "Limiting Science: A Biologist's Perspective." Daedalus 107 (Spring 1978), pp. 37-45.

Because the author was one of the group of scientists that first drew attention to the implications of research with recombinant DNA, his thesis in this essay is of more than passing interest: "I wish to argue that the traditional pact between society and its scientists in which the scientist is given the responsibility for determining the direction of his work is a necessary relationship if basic science is to be an effective endeavor. ... society, while it must determine the pace of basic scientific innovation, should not attempt to prescribe its directions." Baltimore emphasizes that his arguments pertain only to basic research and not to the technological applications of science.

Bennett, William. "Facts, Science, and Common Lives." Harvard Magazine 80 (May-June 1978), pp. 14-16.

To Bennett, poetry rather than science is the more democratic pursuit. Science, by becoming increasingly unintelligible and more exact (and thereby seemingly less certain), has grown away from the common experience and more dependent upon the scientific expert or authority. Bennett observes that the recent controversies over recombinant DNA or laetrile can, therefore, be better understood as expressions of resistance to the authority of scientists than purely as fossil superstitions persisting into an enlightened age" (15).

Blanpain, Jan, with Luc Delesie and Herman Nys. National Health Insurance and Health Resources: The European Experience. Cambridge, Massachusetts: Harvard University Press, June 1978.

A book for those interested in health policy, whether researchers or lay persons concerned with social priorities. In a series of separate analyses, the authors focus on the situation in five different countries--England and Wales, France, the Netherlands, Sweden, and West Germany--describing the process of need recognition and the objectives, scope, and process of delivery of care. For each country, the analysis takes the form of almost an historical narrative, as the authors retrace the road that led to the present situation, noting the choices made and political lessons learned at each step. The last third of the book compares the policies and role of government re health insurance and health resources in each of the countries to the U.S. situation.

Bok, Sissela. "Freedom and Risk." Daedalus 107 (Spring 1978), pp. 115-127.

This helpful essay begins with a brief consideration and cogent criticism of the views that moral problems in science do not exist or do not matter or can be coped with by scientists alone. It then illustrates the nature of the moral choices present in research, discusses the nature of risks involved and the burden of proof regarding such risks, and proposes three approaches to regulating scientific investigations according to the seriousness and complexity of the moral issues they raise.

Brooks, Harvey. "The Problem of Research Priorities." Daedalus 107 (Spring 1978), pp. 171-190.

The author winds his way through the complex processes by which research priorities are set, and examines both the criteria employed ("truth," "utility") and the difficulties that arise when attempts are made to combine "internal" considerations ("scientific merit") with responsiveness to social needs.

Condon, E. U. "Tunneling--How It All Started." The American Journal of Physics 46 (April 1978), pp. 319-323.

A clear, unadorned narrative of one effect of the political climate in 1946-54 on American physics. Condon's account of the early history of quantum mechanical tunneling is also the story of physicist Ronald W. Gurney and the interaction of physics and society.

Culliton, Barbara J. "Science's Restive Public." Daedalus 107 (Spring 1978), pp. 147-156.

Discussion of the various groups of common interest ("publics") concerned with regulation of scientific inquiry. The author does note that this "new sense of public awareness and the climate it engenders [may] have put an end to the myth of the sci ist-scholar free to follow his experimental life wherever it may lead" (150).

Davis, Bernard D. "The Moralistic Fallacy." Nature 272 (March 30, 1978), p. 390.

Focusing on the heritability of human intelligence as an example of research cited as producing potentially dangerous knowledge, Davis attacks the use of moral arguments in forbidding scientific inquiry. He argues that blocking research on moral grounds can become, in effect, "an illogical effort to derive an 'is' from an 'ought'."

Donovan, Arthur, ed. "The History of Science in Undergraduate Education--Three Approaches." Scan 2 (April 1978), pp. 36-41.

Report on a symposium on teaching the history of science held at the 1977 meeting of the History of Science Society. Includes abstracts of papers by Sheldon J. Kopperl, Lois N. Magner, and Stanislaus J. Dundon.

Drew, David E. "Needed: Better Data About Academic Science." [Editorial]. Science 200 (April 28, 1978), p. 385.

"The information system which provides data for policy decisions about U.S. academic science has fundamental flaws." The author, a staff member of the Rand Corporation, cites the difficulty of creating a merged data file due to the variety of definitions and classification schemes used in different agencies.

Durbin, Paul T., ed. Research in Philosophy and Technology. An Annual Compilation of Research. Bibliography edited by Carl Mitcham. Greenwich, Connecticut: JAI Press, Inc., 1978.

This volume, the first of an annual series, is divided into three parts: I. Method, Descriptive Frameworks, and a Practical Program for Philosophy of Technology (Joseph Margolis, Robert E. McGinn, Joseph Agassi); II. The University of Delaware Conference, 1975 (Paul T. Durbin, Albert Borgmann, Willis H. Truitt, Kai Nielsen, Edmund Byrne, Robert E. McGinn, Joseph Agassi, Stanley R. Carpenter, Carl Mitcham); III. Review and Bibliography (Carl Mitcham and Jim Grote).

Ellison, Craig W., ed. Modifying Man: Implications and Ethics. Washington, D. C.: University Press of America, 1977.

A collection of articles examining the implications of human engineering technology; based on presentations at the International Conference on Human Engineering and the Future of Man, co-sponsored by scientific, legal and theological organizations. Special sections address genetic, electro-chemical and psychological engineering, as well as evangelical and theological perspectives.

Elstein, Arthur S., Lee S. Shulman, and Sarah A. Sprafka, et al. Medical Problem Solving: An Analysis of Clinical Reasoning. Cambridge, Massachusetts: Harvard University Press, 1978.

In the past few decades of medical school curriculum reform, educators soon discovered that it was "one thing to talk about the importance of teaching

medical students to solve clinical problems and quite another to define specifically what was meant by the term problem solving" (2). This book reports on the Medical Inquiry Project (1969-73), a program of research on medical problem solving. The Project sought to identify relevant intellectual processes, generate theories, and develop instructional materials and methods. While the book is largely a report on Project procedures and findings, it also contains descriptive summaries of most of the related research and general discussion of the conceptual processes of experienced physicians and medical students, as well as a concise chapter of conclusions, implications, and suggestions for future research.

EVIST Resource Directory. Washington, D. C.: American Association for the Advancement of Science, 1978 (Available free of charge: Office of Science Education, AAAS, 1776 Massachusetts Avenue, N.W., Washington, D. C. 20036).

A directory of programs and courses at U.S. institutions in the field of ethics and values in science and technology.

Geison, Gerald L. "Pasteur's Work on Rabies: Reexamining the Ethical Issues." The Hastings Center Report 8 (April 1978), pp. 26-33.

After delineating the ethical issues raised by the nature of rabies itself, the author focuses on the ethical issues specific to Pasteur's period and historical role. The analysis is based primarily on the contemporary literature about Pasteur's work and treatment because, as Geison insists, "ethical analysis... must be specific not only to the substance of a situation but also to its historical context." Although Geison ultimately concludes that "we do have some basis for doubting the ethicality of [Pasteur's] earliest human trials," this painstaking analysis clearly exposes the dangers of adopting "normative stances vis-a-vis historical actors."

Gingerich, Owen. "Circumventing Newton: A Study in Scientific Creativity." The American Journal of Physics 46 (March 1978), pp. 202-206.

As the author describes a specific case study used in a natural sciences course for non-science students, he explores the affinity between the creative impulse in art and science, arguing that the common aspects are quite ambiguous.

Gould, Stephen Jay. "Morton's Ranking of Races by Cranial Capacity." Science 200 (May 5, 1978), pp. 502-509.

S. G. Morton, a prominant mid-19th century physician and self-styled "objective empiricist," amassed the world's largest pre-Darwinian collection of human skulls, measured their capacity, and produced results that conformed to the prevalent view of Caucasion superiority. The author has reanalyzed all of Morton's raw data (published by the physician) and finds that "they are a patchwork of assumption and finagling, controlled, probably unconsciously, by [Morton's] conventional a priori ranking..." Categories of error found by Gould include: 1) "favorable inconsistencies and shifting criteria;" 2) "procedural omissions that seem obvious to us;" 3) "slips," e.g., obvious errors, explicable only by their conformity with expected results; 4) "convenient omissions," and 5) miscalculations. Gould finds

"no indication of fraud or conscious manipulation" in the case, and suggests that such "selective amnesia," unconscious finagling, is a common problem in science.

Graham, Loren R. "Concerns About Science and Attempts to Regulate Inquiry." Daedalus 107 (Spring 1978), pp. 1-21.

Provides a "taxonomy" or "typology" of concerns about science and technology, illustrated by contemporary and historical examples. The objective of the classification is policy-oriented: to facilitate assessment of "the validity of each concern and address the problem of limits or regulation in a more specific and informed fashion." Graham argues that "at the present time there is a particularly strong case for discussing separately the 'rational variable' in the complex cluster of contemporary concerns about science..." "If we dismiss all concerns about science as 'irrational,' we will not be listening to some important debates."

Gregory, Anita. "Anatomy of a Fraud: Harry Price and the Medium Rudi Schneider." Annals of Science 34 (September 1977), pp. 449-549.

Examines the background and implications of a significant controversy in the history of parapsychology: a 1933 claim of fraud issued by a psychic researcher toward a medium. Drawing from many unpublished sources, this paper touches on a variety of issues raised by the controversy; these range from the design of experiments in an unorthodox area of science, through the relation between fringe and established science, to the role of science popularizers and the ethics of science.

Grosch, Herbert R. J. "Synthetic Chicken." Communications of the ACM 21 (April 1978), pp. 257-258.

An editorial by the President of the Association for Computing Machinery describing provisions in the ACM Articles of Incorporation that prohibit lobbying by the Association. These provisions were recently used to table ACM Council discussion on a proposed resolution to prohibit holding ACM meetings in states that have not ratified the Equal Rights Amendment.

Hall, R. Cargill. Lunar Impact. A History of Project Ranger. Washington, D. C.: National Aeronautics and Space Administration, 1977. Available from the Superintendent of Documents, U. S. Government Printing Office. $6.25. Stock number 033-000-00699-3.

The first close-up photographs of the moon, obtained in 1964, were a result of Project Ranger, conducted from 1959 to 1965 by NASA and the Jet Propulsion Laboratory of the California Institute of Technology. This history of the project, emphasizing the management as well as the technical difficulties encountered, sheds light on the ways in which policies for science are developed in the executive branch of government.

Heijder, Alfred, and Herman van Geuns. Professional Codes of Ethics. London, England: Amnesty International Publications, 1976.

A 32-page booklet that explores ethical codes related to the use of torture or cruel or inhuman treatment. Two essays specifically address the responsibilities of the medical profession and several draft codes or resolutions from the United Nations and other international organizations are included as appendices. Available for $1.00 from Amnesty International, 53 Theobald's Road, London WC1X 8SP England.

Hodgkin, A. L., A. F. Huxley, W. Feldberg, W. A. H. Rushton, R. A. Gregory, and R. A. McCance, eds. The Pursuit of Nature. New York: Cambridge University Press, 1977.

A collection of "informal essays on the history of physiology," written as part of the 1976 centenary celebration of the Physiological Society by authors who were both eye witnesses of and contributors to major developments. [See the review by John W. Moore, "Physiologists' Recollections," in Science 200 (April 21, 1978), p. 304].

Holden, Constance. "ABASS: Social Sciences Carving a Niche at the Academy." Science 199 (March 17, 1978), pp. 1183-1187.

This article chronicles the rapid growth of the social science arm of the National Academy of Sciences--the Assembly for Behavioral and Social Sciences (ABASS)--which emerged from a 1973 reorganization of the NAS.

Holden, Constance. "Court Rules GE May Patent New Microorganism." Science 199 (March 17, 1978), p. 1184.

For the second time within a year, the U.S. Court of Customs and Patent Appeals, in Washington, D. C., has ruled that biological matter is patentable. The rulings have generated considerable controversy about whether animate matter can be patented.

Holton, Gerald. "Epilogue." Daedalus 107 (Spring 1978), pp. 227-234.

Holton argues that the struggles over the limits and boundaries of science have only just begun and are "inevitable and perhaps overdue." He attributes the vitality of the debate to 1) the "visibility" of science and its practitioners, 2) the persistence of old credos in science, 3) new institutional forms for dealing with the problem of limits, 4) changing perceptions of expectations and debts by both science and society, and 5) changes from an "ideology of progress" to a new "ideology of limits," an alteration mechanically and intellectually linked to new conceptions of progress within science itself.

Hutt, Peter Barton. "Public Criticism of Health Science Policy." Daedalus 107 (Spring 1978), pp. 157-169.

Although concerns about undue restrictions on freedom of inquiry are being voiced with increasing frequency by some members of the scientific community, the author--an attorney--contends "that scientists today enjoy greater

freedom of inquiry than ever before in history...." However, he warns that this situation will deteriorate unless scientists attend, seriously and rigorously, to public criticisms of science--no matter how unwarranted and naive such criticisms may appear. The bulk of this article is devoted to a "catalogue" of criticisms of science, particularly health science policy. The list is admittedly neither selective nor analytic, and no sources are cited; the author intends simply "to present a list of public criticism, not to debate the merits of the issues involved."

Kargon, Robert H. Science in Victorian Manchester. Baltimore, Maryland: Johns Hopkins University Press, 1978.

A study of the development of science and scientific institutions in a significant urban locale.

Kopelman, Loretta. "Ethical Controversies in Medical Research: The Case of XYY Screening." Perspectives in Biology and Medicine 21 (Winter 1978), pp. 196-204.

Explores the value conflicts surrounding the XYY screening controversy and discusses the "unique" problem of consent involved in XYY research testing of infants.

Lehmann, Phyllis. Cancer and the Worker. New York: New York Academy of Sciences, 1977.

This practical book is designed to inform workers and managers about what is and is not known about occupational cancer and about the issues involved in its control. Adapted from the proceedings of a New York Academy of Sciences conference on occupational carcinogenesis, the book includes discussions of several classes of cancer hazards, techniques for prevention, risk assessment, the worker's right to know, as well as a glossary of cancer terms.

McDermott, Walsh. "Medicine: The Public Good and One's Own." Perspectives in Biology and Medicine 21 (Winter 1978), pp. 167-187.

Discussion of issues involved in current debates over the direction of health care policy; in particular, "whether the encounter physician system merits continued major investment or whether some other way, largely based on acquiring good habits of health, represents a better investment opportunity for the long run" (184). Author advocates a concerted R & D effort on "the actual substance of medical practice."

Marx, Leo. "Reflections on the Neo-Romantic Critique of Science." Daedalus 107 (Spring 1978), pp. 61-74.

An essay on neo-Romantic criticism of the legitimacy of science, concentrating on the reaction of the late 19th century and with special attention to the writings of Theodore Roszak.

Metzger, Walter. "Academic Freedom and Scientific Freedom." Daedalus 107 (Spring 1978), pp. 93-114.

Debates over the regulation of science frequently include references to academic freedom. In an effort to enlighten this aspect of the debates, this essay "traces the development of conventional notions of academic freedom during the early nineteenth century in Germany and later in the United States and shows us how little they offer in the way of a general defense for freedom of scientific investigation in its twentieth century form" (R. Morison, "Introduction" to the Spring 1978 Daedalus, p. x).

Morison, Robert S. "Misgivings about Life-Extending Technologies." Daedalus 107 (Spring 1978), pp. 211-226.

Morison considers the social and ethical consequences of policies directed toward prolonging life and advocates early application of technology assessment to, in part, forecast the consequences.

National Science Foundation. Reviews of Data on Science Resources, "Scientific and Technical Personnel in Private Industry, 1960-70 and 75." Washington, D. C.: U. S. Government Printing Office. $.80; Stock number 038-000-00-361-5. (Write Superintendent of Documents, U. S. Government Printing Office, Washington, D. C. 20550).

Results of a 1975 survey (with updates) on employment of scientists, engineers, and technicians in private industry. Shows employment levels by industry, occupation, and function, and examines trends in private industry employment of scientists and engineers from 1950-1975.

Osborn, June, ed. Influenza in America, 1918-76: History, Science and Politics. New York: Neale Watson, 1977.

Accounts of the efforts in 1976 to avoid a pandemic of 1918-type flu, with analyses of the political and social questions involved and the accompanying legislative debates and bureaucratic problems.

Perl, Martin L., ed. Physics Careers, Employment and Education. New York: American Institute of Physics, 1978.

Proceedings of a conference on Changing Career Opportunities for Physicists, held at Penn State, 1-3 August 1977. The book contains 44 papers or commentaries on the dynamics of education, funding and employment, on specific non-academic or public policy careers, and on appropriate changes in physics education. An interesting mix of statistics, career anecdotes and thoughtful analysis of a complex situation.

Philosophy of Science 44 (December 1977). Special section on "Value Issues in Science, Technology, and Medicine."

Four essays exploring various facets of value issues in science, technology, and medicine: "Moral Autonomy and the Rationality of Science" by James C. Gaa; "Health as a Theoretical Concept" by Christopher Boorse; "Philosophical

Issues in Technology Assessment" by Stanley R. Carpenter; "Discussion Review: Justice, Theory, and a Theory of Justice" by Marcus G. Singer.

Price, Don K. "Endless Frontier or Bureaucratic Morass?" Daedalus 107 (Spring 1978), pp. 75-92.

A cogent and lucidly written analysis of some of the underlying causes of the recent increase in legal and administrative constraints on university research scientists. While scientists often seem inclined to blame "undue constraints" on politicians, anti-intellectuals, and "a bureaucracy dominated by an overmighty executive," Price exposes more probable causes through his examination of science-government relations over the past 30 years. He concludes with an outline for a "constructive political strategy"--"a functional approach" which would create an alliance between scientific institutions and "other elements of society which might see some common interest in maintaining a degree of autonomy in a pluralistic society."

Ralston, Anthony. "Report on Two Sessions on Scientific Freedom and Human Rights at the Annual Meeting of the AAAS, Washington, D. C." Communications of the ACM 21 (April 1978), p. 345.

Brief report on AAAS sessions from viewpoint of Chairman of ACM Committee on Computers and Public Policy.

Reed, James. From Private Vice to Public Virtue. The Birth Control Movement and American Society since 1830. New York: Basic Books, 1978.

In this detailed study of the birth control movement in America, the author contends that both the development and acceptance of birth control techniques were limited more by social values than by scientific and technological understanding.

Reiser, Stanley Joel. Medicine and the Reign of Technology. New York: Cambridge University Press, 1978.

In addition to tracing the development of key advances in medical technology--including the microscope, the stethoscope, and the electrocardiograph--the author maintains that these technologies have altered the patient-physician relationship and have influenced the provision of medical care and treatment. Reiser fully acknowledges the value of innovations but places particular emphasis on the "costs," or disbenefits, of the "growing supremacy of technology" in medicine. An extensive bibliography is included.

Rescher, Nicholas. Scientific Progress. Pittsburgh: University of Pittsburgh Press, 1978.

The central thesis of this analysis of the determinants of the rate of scientific progress (defined as the number of "first-rate findings") is that the binding constraint is economic. [See the essay-review by Richard Levin in Science 200 (May 12, 1978), pp. 639-640].

Scholarly Freedom and Human Rights. Great Britain: Barry Rose Ltd., published for the Council for Science and Society in collaboration with the British Institute of Human Rights, 1977. (Barry Rose Ltd., Little London, Chichester, Sussex, PO19 1PG; Price: $8.00).

Subtitled "The problem of persecution and oppression of science and scientists," this 63-page essay represents the effort of a study group of the British Council for Science and Society to determine what can be done to protect scholarly freedom on the basis of the existing norms and principles which comprise "International Human Rights Law." Includes sections on Science and Oppression, International Human Rights Law, Rights and Freedoms of Special Importance to Science, and Enforcement of Scientists' Rights and Freedoms.

Sinsheimer, Robert L. "The Presumptions of Science." Daedalus 107 (Spring 1978), pp. 23-36.

A well-known advocate of scientific restraint argues that undisciplined acquisition of scientific knowledge may not always serve the ultimate good of society. To Sinsheimer, the restraints he advocates are feasible--physically, logically, and eventually politically--and represent the necessary price for protecting society and, in some sense, preserving science.

Staats, Elmer B. "The General Accounting Office: Appraising Science and Technology Programs in the United States." Interdisciplinary Science Reviews 3 (March 1978), pp. 7-19.

Reviews of the work of the U. S. General Accounting Office, including examples of GAO interaction in specific technical projects.

Steinberg, Eleanor B., Joseph A. Yager, with Gerard M. Brannon. New Means of Financing International Needs. Washington, D. C.: The Brookings Institution, 1978.

As changes in technology, increasing population, and the growing interdependence of nations place additional demands on international organizations, the problems of financing international efforts grow more complex. This book examines potential revenue sources--including international taxes--and the legal, political and administrative considerations involved.

Steinfels, Margaret and Carol Levine, eds. "In the Service of the State: The Psychiatrist as Double Agent." The Hastings Center Report 8 (April 1978), Special Supplement.

Edited transcript of "A Conference on Conflicting Loyalties," cosponsored by the American Psychiatric Association and the Hastings Center, 1977. Topics covered include: moral dile[illegible] in military practice; psychiatrists in prisons; psychiatric institution[illegible] iewed from the perspectives of administrators and advocates; conflicts and professional etiquette; psychiatrists and potentially dangerous patients.

Swazey, Judith P. "Protecting the 'Animal of Necessity': Limits to Inquiry in Clinical Investigation." Daedalus 107 (Spring 1978), pp. 129-146.

As the author points out, the medical profession has long adopted formal rules or principles of conduct; she traces the development of the current codes and surrounding discussions through the nineteenth century and on to the more recent Helsinki declarations and their relation to current efforts at regulation in and through the FDA and NIH.

Temin, Peter. "Review of Chandler's 'The Visible Hand: The Managerial Revolution in American Business.'" The Bell Journal of Economics 9 (Spring 1978), pp. 297-303.

A review of the 1977 book by Alfred D. Chandler, Jr., which includes discussion of the economic and policy impact of technology on American business in the late 19th century.

Thomas, John R. and Ursula M. Kruse-Vaucienne, eds. Soviet Science and Technology: Domestic and Foreign Perspectives. Springfield, Virginia: National Technical Information Service, 1978. $15.00; Request PB 276968/AS.

This report, based on an NSF-sponsored workshop held in November 1976, analyzes the organization and policies of Soviet science and technology and its interaction with other components of the U.S.S.R. system.

Wade, Nicholas. "Congress Set to Grapple Again with Gene Splicing." Science 199 (March 24, 1978), pp. 1319-1322.

Whether Congress will agree upon a bill to govern gene splicing research (recombinant DNA) in its current session is open to question. This article analyzes controversial provisions of proposed legislation as well as the views of key members of Congress.

Wade, Nicholas. "Contrary to Fears, Public is High on Science." Science 199 (March 31, 1978), pp. 1420-1421.

Recent polls in Europe and the U. S. show no evidence of a decline of public confidence in science.

Wade, Nicholas. "Guillemin and Schally: The Years in the Wilderness." Science 200 (April 21, 1978), pp. 279-282. "Guillemin and Schally: The Three-Lap Race to Stockholm." Science 200 (April 28, 1978), pp. 411-415. "Guillemin and Schally: A Race Spurred by Rivalry." Science 200 (May 5, 1978), pp. 510-513.

An incisive study of the 21-year pursuit, by two competing teams of scientists, to isolate the hormones of the brain. The arduous quest, culminating in a shared Nobel prize, was marked by single mindedness, operation on a semi-industrial scale, open rivalry, and an "active lack of cooperation between the two teams."

Walsh, John. "Historian of Science States Case for Catching Up on Basic Research." Science 199 (March 1978), pp. 1188-1190.

A synopsis of Derek Price's testimony at science subcommittee hearings held at the 1978 AAAS meeting. A pungent restatement of traditional arguments for federal support of R & D.

Wechsler. Judith. On Aesthetics in Science. Cambridge, Massachusetts: The MIT Press, 1978.

Essays by art historians, physicists, mathematicians, psychologists and others address the deep and delicate aesthetic nature of the concepts, models and theories of science. Aesthetic judgments in science, the authors remind us, may reside within a cultural or personal context as well as in the technical structure of science. Several of the essays are concerned with the aesthetics of the scientific concepts or theories themselves (Cyril Smith, Philip Morrison), others (such as those by Arthur Miller, Seymour Papert, Howard Gruber) address the individual aesthetic judgments involved in conceptualizations of scientific ideas. Geoffrey Vickers endeavors to arrange the fields of potential knowledge, thereby to describe our ability "to impose, recognize, and combine forms," an ability he argues is distinct from that of logical deduction and analysis.

White, Lynn, Jr. "Science and the Sense of Self: The Medieval Background of a Modern Confrontation." Daedalus 107 (Spring 1978), pp. 47-60.

A compelling, precisely-executed essay on the relation between individual self-awareness and science in the context of man's religious beliefs. White takes us from the eleventh century to the twentieth in an unforgettable journey through the development of the scientific community's professional legacy and mankind's image of Nature and scientific inquiry.

Woodcock, John. "Literature and Science Since Huxley." Interdisciplinary Science Reviews 3 (March 1978), pp. 30-45.

An evaluation of Aldous Huxley's Literature and Science (1963), followed by a review of the critical contributions since Huxley in the area of literature and science, i.e., Jacob Bronowski, B. F. Skinner, and C. P. Snow. Woodcock reviews science fiction, poetry and essays exploring new views of nature and their social corollaries.

Worster, Donald. Nature's Economy. The Roots of Ecology. San Francisco, California: Sierra Club Books, 1977.

An analysis of the development of ecology in England and America by an intellectual historian who approaches scientific ideas as products of specific cultural conditions.

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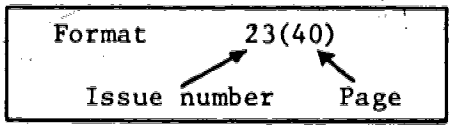

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The Newsletter on Science, Technology & Human Values is a quarterly review of issues, actions and educational activities concerning the ethical implications and social consequences of science and technology.

Editorial coverage includes:

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- Ethical issues and problems which arise in the course of scientific research and technological development, including those encountered by scientists and engineers in their professional capacities;
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- Issues pertaining to the public understanding of science and technology.

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RESEARCH REPORTS describe on-going or recently completed projects in the subject-areas described above; appropriate length is 5-10 pages, double-spaced.

ARTICLES and COMMENTARIES ON ARTICLES are refereed. ARTICLES should be limited to 25 pages, double-spaced; COMMENTARIES should be no longer than 10 pages, double-spaced. To facilitate blind-review, authors are requested to place identifying information on a separate sheet. Papers should be submitted in duplicate. Prospective authors are invited to communicate with the editors prior to formal submission of articles.